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AN  
ESSAY  
ON  
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AN  
ESSAY  
ON  
FIRE.

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*By MARK AUGUSTUS PICTET,*

PROFESSOR OF PHILOSOPHY, AND MEMBER OF  
THE SOCIETY FOR THE ADVANCEMENT OF  
ARTS AT GENEVA.

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TRANSLATED FROM THE FRENCH,  
UNDER THE INSPECTION OF THE AUTHOR,

BY W. B. M. D.

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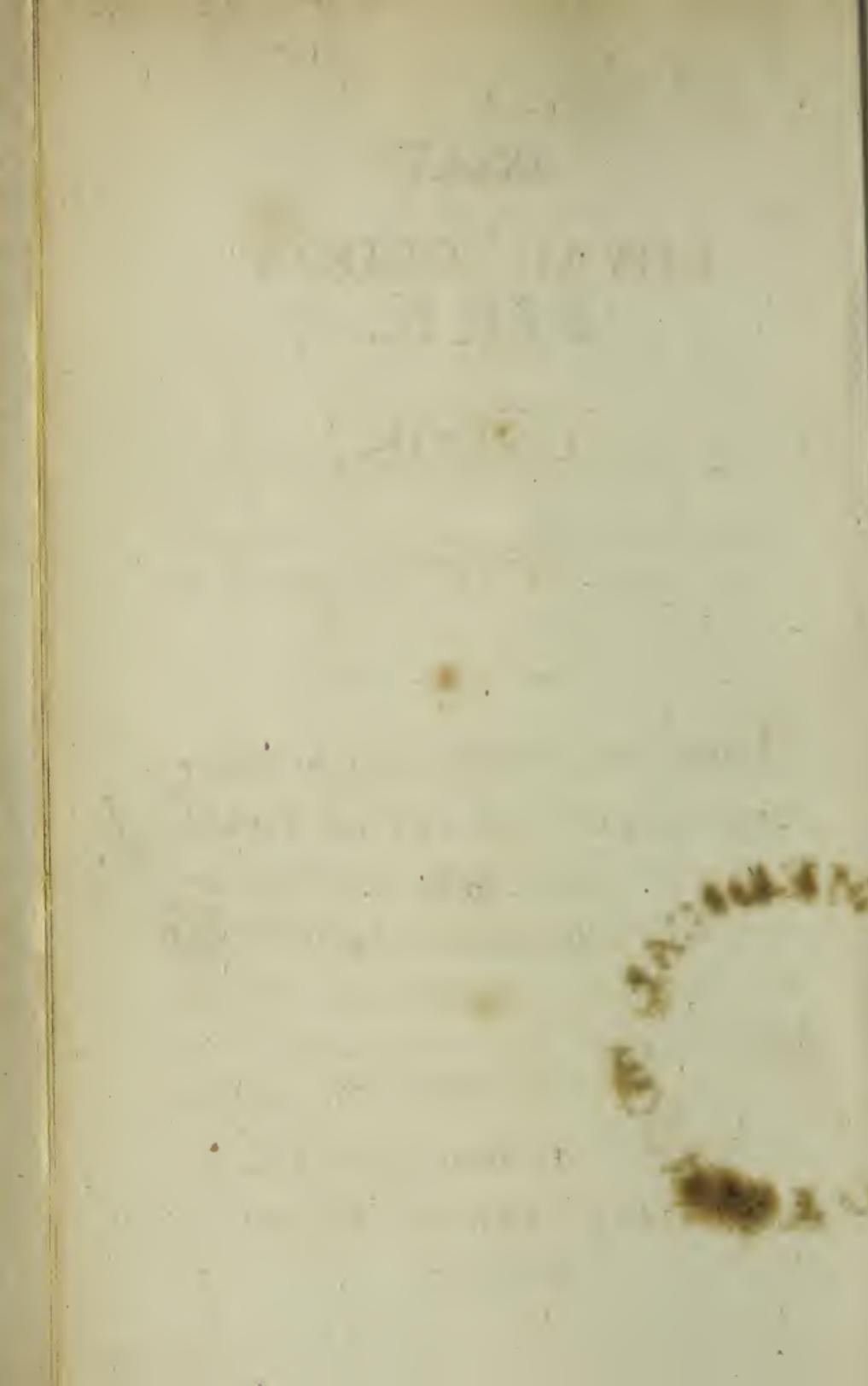
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TO THE  
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OF  
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GENTLEMEN,

THE lovers of science in every country owe you extensive obligations. Your labours for the advancement of natural knowledge, the rigorous exactitude which characterises your enquiries, the numerous and brilliant discoveries to which they have given birth, are benefits which entitle you to the

the admiration of your contemporaries, and to the gratitude of posterity. The homage of my feeble productions is not an offering to which I attach any merit, but a debt that I endeavour to pay.

I have the honour to be,

With the most profound respect,

GENTLEMEN,

Your most obliged

And most devoted

Humble servant,

M. A. PICTET.

## PRÉFACE.

THE present Essay, being the fruit of labours frequently interrupted, is, in many respects, an imperfect work, and would have been still more so, had it been published two years ago, at which time the chief part of it was digested; but the experiments I have since made have enabled me to insert new facts, and to rectify opinions in which I had confided.

On the other hand it is certain, that in consequence of this delay, I have

I have been anticipated in the suggestion of some of the general ideas contained in the first chapter; Mr. Lavoisier, in the *Elementary Treatise of Chemistry*, with which he has lately enriched the science, having considered the modifications of fire in a manner very analogous to mine. And although certain paragraphs of my work resemble, almost word for word, those of that celebrated chemist, I have preferred letting them remain absolutely as I wrote them, and as they were read by  
Messrs.

Messrs. de Saussure and Senebier, and some other friends, two years before the publication of the treatise just mentioned; as the testimony of these learned men, and of the persons who assisted at the particular courses of lectures, in which, during the last three years, I developed the theory, whose abridgement is included in the first chapter of this Essay, will secure me from every suspicion of plagiarism; and I am too much flattered by finding my ideas coincide with those of that learned academician, to deny myself the pleasure

sure of remarking it to my readers.

Having so long retarded this publication, perhaps I ought still to have deferred it; as it will be seen that the experiments comprised in the second chapter, required to be repeated in other circumstances; and that those in the sixth chapter ought to have been varied in all the known aeriform fluids; but my particular situation not permitting me either to dispose of my time as I wished, or to foresee with certainty at what period I might be able to complete

the work, I have preferred publishing *this Essay* at present, intended rather to offer new objects of enquiry to philosophers, and to point out certain tracks in the experimental line, than to remove every difficulty.

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# CONTENTS.

CHAP. I. Introduction—Uncertainty concerning the Nature of Fire—Its Modifications may be considered under four Points of View—Liberated Fire—Specific Heat—Latent Heat—Fire in a state of Combination, or combined Fire      p. 1

CHAP. II. Fire and Light have some analogy—Propagation of Fire in an horizontal Plane—Reasons for supposing in it a Tendency upwards—Apparatus for verifying it by Experiment—Confirmation of this Supposition.      -      p. 50

CHAP. III. Various Experiments on Heat—Description of the Apparatus employed—Effect of the Colour and Nature of Surfaces relative to their Reflection of Heat—Experiment on the refraction of Heat—  
Its

## CONTENTS.

Its Velocity — Apparent Reflection of Cold - - - p 85

**CHAP.** IV. Description of the Apparatus made use of for observing the Transmission of Heat through some elastic Fluids - p. 124

**CHAP.** V. Preliminary Experiments — Effect of the Light of burning Tapers reflected upon a blackened Thermometer — Influence of Daylight — Examination of the Effect of the sides of the Globe upon the calorific Emanation of the Wax Tapers, and of the mean Heat of the Air within during the mean Continuance of these Experiments — The Advantages of this Apparatus for manometrical Experiments.

p. 138

**CHAP.** VI. Experiments in the dry and moist Vacuum filled with the Vapour of Ether — With electric Fluid - - - - p. 158

**CHAP.**

## CONTENTS.

CHAP. VII. Experiments relating to Evaporation, and to Hygrometry in general - - - p. 201

CHAP. VIII. Experiments concerning the Temperature of the Air at different heights — Remarkable Circumstances which these Experiments present, and Consequences deducible therefrom - - p. 244

CHAP. IX. Experiments on Heat produced by Friction - - - p. 283

In page 128, line 1, after *dryness* insert *below*.

ESSAY

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# ESSAY ON FIRE.

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## CHAPTER I.

*Introduction—Uncertainty concerning the Nature of Fire—Its Modifications may be considered under four Points of View—Liberated Fire—Specific Heat—Latent Heat—Fire in a State of Combination, or combined Fire.*

§ 1. THE actual progress of those Sciences, whose object is the study of nature, promises rapid success; and

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the numerous discoveries, which are the consequence of it, and which distinguish the present century, disclose how much we have to expect from the future. It has at length been felt, that we ought to attack the tree of Science, not by the trunk, but by the branches; that we ought to accumulate facts, vary them in every possible manner, and only recur to Systems and Theories, as to methods of arrangement, as to a faint light, which, without doubt, may conduct us, in the search of great truths, to a certain point, but which may also lead us astray.

§ 2. Invited by taste and profession to the study of natural Philosophy, I have thought it my duty to unite my efforts with those of my cotemporaries, to contribute to the increase of the mass of facts, which support our knowledge

ledge in this branch of Science; and I publish my principal experiments, with their results, that these materials may be ready to be employed by more dexterous artists.

§ 3. Fire, some of whose modifications are the object of the experiments of the present Essay, may be cited in proof of this humiliating truth, that our positive knowledge is reduced to facts. The antients had strange notions of the nature of this element: the heat they called *celestial*, that of combustion, of boiling water, of fermentation, and animal heat, were, according to them, so many different kinds of heat. These errors are now abandoned; but the greatest philosophers of this age do not yet agree in their opinions concerning the nature of fire. Some consider it as a simple

modification of bodies, as a vibratory commotion in the particles which compose visible aggregates, and they attribute change of temperature to variations in the intensity of these vibrations. According to other philosophers, fire is a peculiar fluid, *sui generis*, which passes easily through all bodies, which is prodigiously expansible, and which, in consequence of its elasticity, or some other cause, dilates all bodies whose texture it penetrates. Amongst those, who regard fire as a substance, some believe it an elastic fluid, universally diffused, capable of vibration ; and by these vibrations, they explain its principal effects. Others consider it as a particular radiant emanation, emitted by hot bodies, and expanding around them according to certain laws. Some believe

lieve it without weight ; others imagine that, far from being ponderous, it lightens bodies with which it is united. Some esteem it a simple being, a true element ; others believe it a compound. This opposition between authorities equally worthy of confidence, and the little concord concerning facts themselves, prove but too well the discouraging truth I have just advanced. Each of the two fundamental opinions has its advantages and its difficulties ; both explain, almost equally well, the phenomena ; and, in the present state of this science, it is not easy to decide, whether fire is matter, or only a simple movement in matter. The expression of *quantity* applied to the cause of heat, will therefore apply to a certain matter, or to a

certain movement, according as either of these opinions shall be embraced.

Its influ-  
ence in na-  
ture.

§ 4. I confess I would rather, with one of the first chemists of our times\*, consider fire, not only as a substance, but as the universal agent, whose power constantly modifies that general law of affinity, which tends to unite the molecules of matter in order to form aggregates. I am pleased in considering the same body now solid, now liquid, now elastic fluid, according as the expansive force of fire is weaker, equal, or stronger than that of the affinity, which tends to unite its integral molecules. This idea seems to me too exquisite, too conformable to the course of facts, and the simplicity of nature, to be only an hypothesis.

\* Mr. Lavoisier. Mr. De Saussure has long taught the same theory in his Lectures.

§ 5. Heat,

§ 5. Heat, properly speaking, is that sensation which the presence of fire occasions in an animate body. The state of an inanimate body, when it contains fire, is also distinguished by this word: for, in this sense, we say, the heat of red-hot iron. Custom has likewise consecrated an acceptation of this word, still more distant from its true sense. Many writers, as well in our own as in the English language, signify the cause by the name, which should have been appropriated exclusively to the effect. They call fire itself, considered in a state of liberty, *heat*, and sometimes *the matter of heat*. It has likewise been denominated *fluide igné*, and *fluide calorifique*\*.

\* Igneous fluid and calorific fluid.

Finally, the celebrated chemists, who have lately proposed a new nomenclature, call it *caloric*; a happy expression, which, without doubt, will be universally adopted.

It may be  
considered  
under four  
points of  
view.

§ 6. Reflecting on the igneous phenomena presented by the whole of the discoveries made until the present day, it appears that they may be classed under these four heads, viz. *liberated fire*, *specific heat*, *latent heat*, and *chemical or combined fire*; which shews us this element under so many different points of view. We may consider it, <sup>1st. Libera-</sup> first, as produced or developed in a <sup>ted fire.</sup> certain place by any cause whatsoever, and tending to expand itself around the centre or *focus* as an invisible emanation, which moves according to certain laws, with a certain velocity; which penetrates more or less readily

readily all substances it meets; which occasions the sensation of heat in animated bodies, and dilates or augments the dimensions of almost all others. This is the *igneous fluid, free caloric, thermometric fire*, and, according to others, *heat*.

§ 7. In proportion as fire by this motion penetrates any substance, the bulk of this substance is increased; and this is the most general indication of the presence of fire. To appreciate its gradations, we employ substances, whose augmentation of volume is as proportionable as possible to the increase of heat. In this respect, Mercury has been happily chosen. According to this principle we may conceive the use of Thermometers: they always admit the presence of liberated fire, and indicate

pretty

pretty exactly its increase or diminution within certain limits.

Its most constant character.

§ 8. The most constant character of fire in its state of liberty is a continual tendency to equilibrium, or to issue from the place where it is in the greatest *tension*, towards that where it experiences less. (I shall presently explain what I mean by *tension*.) Thus all bodies, when heated, are, in some degree, in a forced state; the fire remains there chiefly, because it is repressed by the ambient fire. In this point of view this fluid cannot, in general, be restrained or *cohibited*, but by itself: the equilibrium which sometimes results therefrom is called *temperature*.

What is indicated by the thermometer.

§ 9. It is essential to conceive clearly, from the commencement, what sort of indication the thermometer

ter affords us. Let us suppose a thermometer in a vessel full of water, with abstraction of the surrounding air: the water and the mercury of the thermometer contain fire, which tends to quit both. If this tendency be equal in each, the mercury will neither rise nor fall, on plunging the instrument into the water: there will be equilibrium between these equal *tensions* \*, and the thermometer will shew, by the degree observed on its scale, the *temperature* of the liquid. If the *tension* of the fire had been greater in the wa-

\* I have chosen this word *tension*, because it has been appropriated to this idea by Mr. Volta, who has used it in an analogous sense relatively to the electric fluid; but if I did not fear being accused of *neologism*, I should much prefer the word *expansiveness* (*expansiveté*) which renders much more exactly my idea.

ter than in the thermometer, the heat would have been directed to the instrument, until it had acquired a *tension* sufficient to resist a new increase. The thermometer having risen as long as the introduction of fire into the substance of which it is composed, continued, would have stopt at the point of equilibrium, and would have shewn the temperature of the liquid. Again, if the *tension* of the fire had been less in the water than in the thermometer, the excess would have been communicated by the thermometer to the water, until they had acquired an equilibrium : the thermometer would have descended as the heat abandoned it, and ceasing to descend, when the *tension* of the fire in the instrument equalled that, which existed in the ambient

ambient liquid, it would have again indicated the temperature of the water.

§ 10. This *tension* of liberated fire, on what it depends. in whatever substance it has penetrated, depends on two causes : on the degree of its real accumulation, or absolute density, and on the greater or less faculty of the substance in which it is accumulated to restrain or retain it ; faculty which is called its specific heat, and of which we shall by and by give an explanation. The *tension* is in direct ratio of the density, and inverse of the specific heat.

Thus we see how far thermometers are from teaching us any thing respecting the absolute, or even relative quantity of heat contained in the bodies, whose temperature we examine by these instruments. They only shew us the translations of the igneous fluid,

fluid, and subdivide, into nearly equal aliquots, a certain part of the entire scale of heat, whose real extent is unknown.

Precaution  
in the use of  
thermome-  
ters.

§ 11. As the thermometer almost always gives or receives heat, in the manner in which we use it to measure the temperature of bodies, it is necessary, if we desire that the experiment should be physically exact, that the instrument should have so small a volume, that the quantity of fire given or received may be neglected without any sensible error.

2nd. State  
of fire, spe-  
cific heat.

§ 12. Let us at present suppose a certain focus, from whence issues a constant and uniform calorific emanation ; let us place, at equal distances around it, several substances of the same nature and size ; these will all be penetrated by the calorific emanation,

nation, and their temperature will rise by equal degrees, and will cease to rise when the fire shall have acquired, by its accumulation, a tension sufficient to enable them to resist "a new augmentation ; but if we place around this focus at equal distances different substances of equal mass, or equal weight, and of different nature ; as for instance, a pound of water, a pound of glaſs, a pound of mercury, &c., the fire will penetrate them all, and they will all finally acquire the same temperature, but in different spaces of time, and by dissimilar degrees. This may depend upon two causes difficultly separable : the different permeability of these substances to the matter of fire, or their faculty of conducting heat, in virtue of which a longer or shorter time is required

quired to penetrate their texture; and the different faculty of these bodies to contain, or more properly speaking, to retain the *free caloric*. The more this faculty or *capacity*, as it is called, or *affinity*, as I think it ought to be called, is considerable, the more it will permit the real accumulations of fire, before the term of equilibrium, resulting from that accumulation, takes place: so that when this equilibrium shall obtain, although it may indicate an equal tension in the matter of fire, it will not indicate, nor any thing near it, that this equal tension has for cause equal accumulations of heat, in the several bodies equal in mass.

Examples drawn from humectated substances. § 13. I shall explain these considerations, of which it is important to have

have a very clear notion by another example.

Let us plunge into a basin of water, at the same instant, a pound of dried sponge, a pound of blotting paper, and a pound of some porous wood. At the expiration of a certain time, these substances will be equally wet, and will have imbibed all the water they are capable of retaining. The blotting paper will have been first penetrated by the water, as being very permeable to this liquid. The sponge will be less quickly penetrated, perhaps, for two reasons: first, because dried sponge is less permeable than the paper; secondly, because it has a greater capacity of imbibing water, and consequently, *ceteris paribus*, it will require more time to be saturated with water than the paper. Lastly,

the wood will require a much longer time to be fully humected, although it has less capacity, being less permeable to water than the other two substances.

When we take these three substances out of the water, they will be wet, apparently, in the same degree, as well externally as internally ; but they will be far from containing the same quantities of water. The paper will have absorbed more than the wood, and less than the sponge.

This is what happens to various substances of equal masses and different natures, plunged into a calorific atmosphere, and heated to a like temperature. And I believe this comparison so exact, that I esteem the affinity of the water which wets, identical to the affinity of the fire which heats,

heats, which evaporates, which, in general, produces all *solutions*\*, and which, as I think, is nothing but the affinity of cohesion, or *physical affinity*, as I call it, in opposition to *chemical* or *elective affinity*.

Could we apply an hygrometer to these substances, which have served us as examples, at the moment they are taken out of the water, this instrument would indicate nothing more, than that they were equally humected; and would leave us ignorant of the quantities of water they contained: In like manner the thermometer applied to substances heated to the same degree, will only shew, that the fire has an equal tendency to abandon them, but will teach us nothing as to

\* Very different from dissolutions.

the absolute or relative quantities of heat which produce this tendency. However, we shall know the relative quantities of water which humected the three substances, if, for example, we exsiccate them to the same degree, in an apparatus proper to collect separately the water, which will abandon each of them.

We may likewise obtain the relative quantities of heat, contained by various substances, heated to the same thermometric degree, if we cool them to the same degree, in an apparatus proper to receive and measure separately the quantity of heat which abandons them during their refrigeration. This may be effected either by means of mixture, or by the ingenious apparatus in which Messrs. Lavoisier and

and De la Place \* have employed, with this intent, the fusion of congealed water.

But yet, by exsiccating to the *same degree*, the humectated bodies, which have served us as examples, we only obtain the *relative* quantities of water, which made them equally wet, and not the *absolute* quantities of water, which they contained; because we are far from having procured a perfect exsiccation. In like manner, by measuring the quantities of heat which escape from the different equi-ponderent substances, heated to the same degree, we only obtain, by an equal refrigeration, the *relative* quantities of heat, which produced in them an equal *tension*, or similar thermo-

\* Mém. Acad. 1780.

metric effect, and not at all the *absolute* quantities of heat they contain; because we are still far distant from a perfect refrigeration \*.

Fire,

\* In my preface I have said that I had the happiness to coincide with Mr. Lavoisier in a remarkable manner. I cannot resist the temptation of recounting to my reader, what this celebrated Chemist has said in his *Elementary Treatise of Chemistry*, vol. I. page 19, which may be compared with what I have said above, two years before the publication of that work, which makes an epoch in the science. "An example" says he, "of what passes in water, and some reflections on the manner in which this fluid wets and penetrates bodies, will render this more intelligible. One cannot, in abstract subjects, too sedulously seek assistance from sensible comparisons. If we plunge into water pieces of wood of different kinds, of equal bulk, a foot cube for example, this fluid will gradually

Fire, considered in this point of view, viz. as accumulating in a

“ ally introduce itself into their pores ; they  
 “ will swell and augment in weight, but each  
 “ piece will admit into its pores a different  
 “ quantity of water ; the lightest and most  
 “ porous will lodge the greatest quantity ;  
 “ those which are compact and close will re-  
 “ ceive only a small quantity. In short, the  
 “ portion of water they receive, will depend,  
 “ likewise, upon the nature of the constituent  
 “ molecules of the wood, on the greater or  
 “ less affinity they may have with water ; and  
 “ very resinous wood, for instance, although  
 “ very porous, will admit but little. It may  
 “ therefore be said, that the different kinds of  
 “ wood have different capacities for receiving  
 “ water ; one may even know, by the increase  
 “ of weight, the quantity they have absorbed,  
 “ but it will be impossible to know the abso-  
 “ lute quantity they contain on their being  
 “ taken out of it, because we were ignorant  
 “ of the quantity of water they contained be-  
 “ fore their immersion.”

greater or less quantity in substances of different natures, but of equal masses, which it penetrates, and in which it acquires an equal tension, has been called by some natural philosophers, *latent heat*, and by the greatest number, *specific heat*. It may be defined with the learned academicians abovementioned, “ the relation of the quantities of heat, necessary to raise, through the same number of degrees of temperature, divers substances of equal masses.” The expression, *specific heat*, appears most happily applied.

This modification had escaped philosophers until our own times. I find the first traces of it in the justly celebrated work of Mr. De Luc, on the Modifications of the

This modification had escaped philosophers until our own times. I find the first traces of it in the justly celebrated work of Mr. De Luc, on the Modifications of the

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fication  
formerly  
unknown.

the Atmosphere\*, published in 1772; and it had been already developed in the lectures given at that period by Dr. Black, of Edinburgh. The united labours of this distinguished chemist, of Messrs. Lavoisier and De la Place in France, of Mr. Wilkie in Sweden, of the Chevalier Landriani at Milan, of Drs. Crawford and Irvine, of Messrs. Kirwan, Watt, and

“ I know not,” says he, “ if we have a just idea of what we call equality of difference of heat in bodies of different natures, when we once penetrate beyond appearances, or the indications of the thermometer. It is very little probable that different bodies, which are denominated equally hot, because they keep the thermometer at the same height, contain a similar quantity of fire, in the same volume or in equal masses.”

Essai sur les Modifications de l'Atmosphère, § 973.

Magellan,

Magellan, in England, have singularly extended this new branch of physics. They have proceeded by experiment, and have taken the specific heat of water as their unit, and as their common standard. Some of

Tables of specific heat these gentlemen have given tables of the specific heat of a great number of substances, both solid, liquid, and aeriform.

They have neglected the consideration of volume. § 15. But are they not blameable for having neglected, in the compilation of these tables, the consideration of the volume of the substances submitted to experiment, and for having referred the specific heat solely to the weight or quantity of matter? It appears to me, that to have had clear ideas of that modification of fire, they ought also to have considered the volume. Thus a pound of air occupies

pies a space about 800 times greater than a pound of water; suppose there was neither air nor water in these two spaces, so very disproportionate, there would be 800 times more fire in the first than in the last, the tension being equal. Let us introduce air and water respectively into these two spaces, and consult the tables of specific heat, we shall see that it only requires eighteen times and a half more fire to raise the thermometer a degree in the pound of air, than in the pound of water, which occupies a space 800 times less. Hence this consideration shews us much more clearly the great relative force with which water retains fire, or its specific heat.

§ 16. This fire, retained in the air by affinity, may be separated to a certain degree from that which is *cohibited* by

by ambient fire, in the absolute space  
 which this same air occupies, under  
 a given pression of the atmosphere.  
 It is in this way that we can account  
 for an experiment mentioned by Mr.  
 Lambert, in his Pyrometry, and which  
 Mr. De Saussure has repeated in a  
 recipient perfectly exsiccated\*. I  
 have also made the same experiment  
 several times. If, after having ob-  
 tained a vacuum in a receiver contain-  
 ing a thermometer, you give a brisk  
 admission to the air of the chamber,  
 heated to the same degree that the  
 thermometer within the receiver indi-  
 cates, it will suddenly rise about two  
 degrees of the scale of 80 parts. I  
 adopt, with Mr. De Saussure, the  
 explication of this phenomenon, as

\* *Essais sur l'Hygrométrie.*

Experi-  
 ment which  
 proves the  
 faculty of  
 the vacuum  
 to contain  
 fire.

given by that celebrated geometrician :  
 " The heat," says he, " that is carried by the mass of air which enters the receiver, joins that which existed already in the same receiver, unattached to any substance, and the rise of temperature is produced by this accumulation of the two quantities of heat in the same space." The contrary happens when you briskly exhaust the receiver; and this second fact confirms the explanation of the former. I have also varied this experiment, by inserting the bulb of a thermometer in the extremity of a condensing pump; when, on forcing briskly the sucker, you condense the air at the extremity of the cylinder in which your thermometer is lodged, you will see it instantly rise about two degrees.

Seems to prove that fire is matter.

§ 17. The same experiment seems to prove that fire is matter, and not a simple movement; because the thermometer, at first cooled by the exhaustion, ascends again in the vacuum to the precise temperature of the ambient air: if it should not be objected, that there still remains sufficient matter in the most perfect vacuum our machines can produce, to occasion, not only heat by vibration in this matter, but also a degree of heat exactly equal to that which is observed in the air and other surrounding bodies. Whereas, by supposing fire a simple, very subtil elastic fluid, which cannot traverse the glass without difficulty, the fact is more happily explained.

What has been understood by the capacity of heat.

§ 18. Some authors have attempted to explain the different specific heat of various substances, by making it depend

pend on what is called their *capacity* of heat, or their faculty of containing fire in such a manner, that it may be (to use a familiar expression) more or less *at its ease*, in their various structures, and may accumulate these in greater or less real quantities, notwithstanding it may exert only the same tension. This expression may be convenient, but it appears to me not very exact, because the only idea it offers is *bulk*, which has been expressly banished in the consideration of specific heats.

§ 19. It appears to me, that by attributing the difference of specific heat, not to different imaginary *capacities*, but to the *affinity*, or, to speak more properly, to the *less repugnance* of different substances for fire in a state

The term  
affinity  
would per-  
haps be  
more suit-  
able.

state of liberty, we should approach nearer to a true explanation, and we should arrange this class of facts under the same laws, which physics and chemistry force us elsewhere to acknowledge.

It is easily conceived that this affinity of bodies with liberated fire, ought to retain it more or less efficaciously in the bodies it penetrates; and as the tension it will exert, can only be the excess of its natural expansive force, over the attractive force of the molecules of the substance it has penetrated, the specific heat of different bodies at an equal tension, or in other words, at an equal temperature, will be in a direct ratio of their forces for retaining liberated fire, or of their affinities with heat.

§ 20. I have said, § 12, that the <sup>Means of separating</sup> permeability of bodies, or their faculty <sup>the permeability from</sup> of conducting heat was difficultly separable from their specific heat. <sup>the specific heat.</sup> There is, however, one means of effectuating it by experiment.

If all bodies were equally permeable to heat, the *spaces of time* employed to raise different bodies, of equal masses, to a certain temperature by the same calorific cause, ought to follow a certain law, relative to the quantity of heat, which has produced this temperature, or to the specific heat itself. Hence, the comparison of the specific heat of various bodies with *the time* required to raise, by the same calorific cause, these various bodies, of equal masses, to the same temperature, would give, I think, when this law should have been once

previously established, the expression of *permeability* disengaged from that of specific heat. I do not believe that any experiments have yet been made with this intent.

*Third point of view, latent heat.* § 21. Considering fire as the sole agent in the two metamorphoses, in which solids are transformed into liquids, and liquids into elastic fluids, it presents itself here under a third point of view, analogous to the preceding; but from which, however, it ought to be distinguished.

The *same* substance, according as it happens to be in any one of these three states, not only possesses a different specific heat, but modifies, even in the act of passing from one to the other, the matter of fire in a very particular manner. We owe this discovery to the immortal Black, who has

has named it *latent heat*. The simple exposition of the phenomena will render this idea clear.

Let us, for example, suppose a <sup>Exposition of this phenomenon.</sup> piece of ice, cooled until a thermometer, placed within it, stands at 10 deg. below freezing point \*, then expose this ice to a constant emanation of heat which arrives by very equal degrees. The thermometer placed in the ice, will rise uniformly from 10 deg. to freezing point, and will there stop, although the calorific emanation continues the same, and ought, apparently, to continue to raise the temperature as before. This current of heat, which arrives unceasingly, and by equal degrees at the ice, is

\* I always mean the ordinary scale of 80 degrees, unless I expressly mention another.

therefore no longer sensible to the thermometer, as soon as it has been thereby raised to freezing point; its effect is limited to making the ice *change its state*, in converting it into water, and during the whole course of this transformation, the thermometer remains stationary at zero.

The fire loses, therefore, in this instance, its faculty of heating; yet the quantity employed, and apparently destroyed, in this transformation, is so considerable, that if a thermometer had been placed in a similar quantity of *liquid water*, at zero, instead of *solid* water at that temperature, it would have been raised in the same time, and by means of the same heat which was employed to melt the ice, to near the 60 degree.

As

As soon as the fusion is completed, supposing the same calorific emanation to continue, the thermometer, now actually in water, will admit the degrees of heat as they arrive; it will rise successively, though somewhat more slowly than before, when the ice began to melt. This difference is owing to the specific heat of water, which is a little greater than that of ice.

The thermometer continues to rise until at length it arrives at the point of ebullition, and there the same phenomenon re-appears. Notwithstanding the continuation of the calorific emanation, the thermometer remains stationary at  $80^{\circ}$ , but the water now *changes its state*. It is transformed into an *elastic fluid*, and the portion of fire, which by its momentary union

with the water, effected this metamorphosis, loses its thermometric faculty. The same thing happens here that we have observed in the preceding case, and an exact equilibrium is established between the afflux of additional fire, and its efflux in the conversion of water into an elastic vapour.

This heat, thus modified by the particular state of aggregation in the molecules of the same substance, in these different states, has been named *latent heat*. It is truly latent or hidden, for it re-appears entirely, and becomes again free caloric, if we reverse these changes; that is, if we convert the elastic fluid into a liquid, and the liquid into a solid.

The heat is  
not chemi-  
cally com-  
bined: § 22. Some philosophers of the first  
distinction have considered fire under  
this point of view, as being chemi-  
cally

cally united with the integral parts of the aggregates\*. I cannot adopt this opinion. One ought not, I think, to call any union chemical, except that which cannot be dissolved, but by the action of chemical affinities. Now, in this instance, the fire has not at all lost that tendency to equilibrium or *expansiveness*, which characterizes it as fire in a state of liberty. The union it contracts is so slight, that the neighbourhood of a cold body is sufficient to destroy it. It loses, it is true, its thermometric and calorific effect, because all its effort, all its *nibus*, if I may be allowed the expression, is employed, is spent in

\* De Saussure, *Essais sur l'Hygrom.* § 188.  
De Luc *Idées sur la Météorologie.* T. I.  
§ 213 and 250.

maintaining the new modification, viz. *liquidity* or *elasticity*, which it has produced, and in resisting the affinity of cohesion, between the integral molecules of the substance with which it was for a moment united, and to which it has now given a new state. The whole quantity necessary to produce this effect, becomes null for every other purpose, as long as this new modification exists. As water, which wets and distends a sponge, seems to have lost those properties which depend upon the action of gravity; it no longer descends, it no longer seeks its level; yet it is not chemically combined, for the least pressure disengages the liquid which is retained by mere *physical* or *cohesive* affinity; so we observe a similar combination in all the modifications of fire,

fire, we have hitherto considered, and we see it issue from air compressed by a condensing pump, in the experiment cited § 16, precisely in the same manner, as water issues from a sponge.

§ 23. This state of fire appears to me so much characterized by that of the substance which it modifies, that I would willingly call the portion of fire employed, as we have just seen, in forming liquids and elastic vapours, the heat of *liquidity* and the heat of *evaporation*, for the same reason that we call the water essential to a substance in a crystalline state, the water of *crystallization*; and I thus separate it entirely from the idea of specific heat, with which it has been often confounded.

4th. Modification. Fire as a principle, or combined fire. § 24. Lastly, fire, without doubt, exists intimately and chemically complete, or combined in bodies, and forms one of

their constituent principles. In this case, it has not only lost all thermometric and calorific faculty, but also that tendency to equilibrium, which it has preserved in all the foregoing modifications ; and it is so firmly connected by the ties of chemical affinity, that no refrigeration can disengage its elasticity. It exists in this state, for example, independent of specific heat, in acids, in permanent aeriform fluids, which all appear to owe to it their elasticity ; and it is only at the moment, when these bodies are decomposed by the effect of elective affinities, in order to give place to new combinations, that this fire is liberated from those ties, that it springs with its

its natural energy, regains a tendency to equilibrium, and becomes heat. But this heat rarely shews itself entirely; in general it partly enters new combinations which conceal it; it becomes *heat of liquidity*, of *evaporation*, and thus escapes our senses. But there are, however, few chemical mixtures, accompanied by decomposition and new combinations, in which the temperature is not changed, and in which there is not consequently a disengagement and absorption of fire.

¶ 25. Two circumstances occur in these mixtures: in one, only a new arrangement of the *integral* parts of the substances mixt, takes place, which occasions the expulsion, or introduction of a certain quantity of heat; in the other, there is a true decomposi-  
tion

It is sometimes disengaged from mixtures, without having been, on that account, truly combined.

tion or new arrangement of the *constituent* parts. Thus, in the mixture of spirit of wine and water, for example, or of water and vitriolic acid, there is a reciprocal penetration of the two liquids, without decomposition of either, and an expulsion of a part of their fire is observed, which becomes sensible heat. The mixture of water with quick lime is of the same kind; there is no decomposition, as I think, but there is certainly a new arrangement of the *integral* parts; and the considerable heat disengaged in this mixture, may as well be attributed to the heat of liquidity in the water, which it loses in passing to a state, in some measure, of solidity in the lime, as to the fire combined with the lime during calcination, and which is disengaged

engaged by the greater affinity of the water with lime. Lastly, the mixture of ice and salt, which decomposes neither, but which produces a remarkable degree of cold, affords us an example of the absorption of heat; because this mixture passing to a state of liquidity, and not having in itself a sufficient quantity of fire to furnish its new form, it suddenly exhausts the bodies, which surround it, of their fire.

In all these cases, we have been speaking of fire physically, and by no means chemically combined. There is no display of affinities, except between the *integral* parts of bodies, and so long as the *constituent* parts are unconcerned, there is no chemical affinity in action.

At other  
times with  
decomposi-  
tion of the  
substances  
mixt.

§ 26. In the other case, the mixture is accompanied with true decomposition, as, for example, when we pour concentrated mineral acids upon oils; and as the substances, in this example, are scarcely evaporable, there is only a very small portion of fire disengaged, which undergoes new connections, and the sensible heat resulting from the mixture is sufficient to ascertain the sudden inflammation of the oil.

This is  
what hap-  
pens in  
combus-  
tions.

§ 27. Combustion, this so common and abundant source of heat, is only a chemical decomposition, in which a part of the atmospheric air loses the fire that entered its composition as an elastic fluid. This fire having acquired a state of liberty, diffuses itself around the burning substances; a part enters into new combinations with the aeriform

aeriform or solid products of the combustion, and the rest dissipates itself as sensible heat.

§ 28. Finally, one might believe that combined fire is sometimes disengaged by a kind of *mechanical* decomposition of the bodies in which it is contained.

One might be tempted to attribute to this mechanical decomposition, those cases in which heat is excited or developed by violent percussion or friction. In particular, I was not far from imputing the heat, disengaged by the ordinary method of striking fire with a flint and steel, in a great measure, to the mechanical decomposition of the ambient air, resulting from the violent contusion it undergoes between two hard surfaces, which strike together in a manner the most favourable

favourable to effectuate this purpose. I founded my conjecture upon the following circumstance: having often observed, with a microscope, the fragments detached from the hammer of a pistol, I had repeatedly snapped in a vacuum, I had never found them in melted globules, like those obtained, by the same means, in the atmospheric air, but always in simple metallic ribbands, painted with the colours of the rainbow. It will be seen hereafter, that more direct experiments have convinced me of my error. I shall therefore range fire, disengaged by friction or percussion, in the class of the modifications of this element purely physical, as may be seen CHAP. IX.

Fire, under the fourth point of view, of which I have just given a sketch, may be called *constituent fire, combined*

combined fire, elementary fire, and it has often been confounded with *latent heat*.

§ 29. Although liberated fire, or fire in a state of liberty, has been the chief object of my experiments, yet as the other modifications, I have just now specified, are likewise interested in them to a certain degree, I thought, that at a moment, when ideas are still unsettled on this capital subject, an endeavour to class the principal facts under a small number of heads, and to present, in the clearest and most concise manner possible, the characters of the different modifications of fire, would not be unacceptable.

## CHAPTER II.

*Fire and Light have some analogy—  
Propagation of Fire in an horizontal Plane—Reasons for supposing in it a Tendency upwards—  
Apparatus for verifying it by Experiment. Confirmation of this Supposition.*

Fire and  
light have  
some ana-  
logy.

§ 30. FIRE, in a state of liberty, has some analogy with light, and differs from it in many respects. Sometimes they appear together, but we often see a strong light without heat ; for example, the rays of the moon collected at the focus of a concave mirror. At other times we have a strong

strong heat without light. It is true, that every time heat arrives at a certain degree of intensity, it is accompanied with light: but it may be said, that this light is only a simple diffusion of that which appears in the act of combustion, without which a great intensity of heat cannot be produced, unless in the case where the optic focus gives heat without combustion; but then it gives, at the same time, a very strong light.

What, in the actual state of our knowledge, we are able to advance as most probable, is, that light and fire are to each other, as a whole to a part: fire may be one of the *components* of light, or light one of the constituent parts of fire, as the celebrated philosopher, already quoted, believes \*.

\* De Luc, Idées sur la Météorologie.

But these systematic discussions would detain us too long from our subject.

Law by  
which fire  
is propaga-  
ted in an  
horizontal  
plane.

§ 31. Fire, disengaged by any of the causes we have touched upon in the preceding chapter, is propagated by radiation from the focus where it was produced. This fact may be explained equally well, by supposing a *real* emanation from the focus, or simple *vibrations* excited at this focus, in the igneous fluid, considered as elastic, and filling space by the same laws as sonorous waves. But the idea of a real radiating emanation offering something more clear to the understanding, I prefer this expression.

It was natural to suppose that this emanation, like all others, which tend from the center to the circumference of a sphere, should diminish in intensity on a given surface, in the inverted

inverted ratio of the squares of the distance. The celebrated chemist, Lambert, however, has confirmed this opinion by experiment. He disposed horizontally, at different distances from the same central fire of charcoal, several thermometers, and took every necessary precaution to render the experiment exact ; and he observed, that their respective ascensions were in the inverse proportion of their distances from the fire.

It is clear, that this law can only have place in an horizontal plane through the air ; for the presence of fire dilating this fluid, and rendering it specifically lighter, it mounts in proportion as it heats, and carries upwards with it the fire, which dilates it ; but this effect being the same at equal horizontal distances, it makes

no alteration, as to the result of the experiment, in this direction.

<sup>Has not fire a natural tendency upwards?</sup> § 32. But does this emanation really radiate in every direction with equal facility? And supposing it to move indifferently towards every region of an horizontal plane, may it not, at the same time, have a greater tendency to mount, than to descend? This question appeared to me, in all respects, worthy of some experimental inquiries. Modern and exact experiments concerning the apparent lightness of water in a state of liquidity, compared with water in a state of solidity, might have made us suspect an *antigravitating* tendency in fire, and these conjectures appeared to merit confirmation or refutation.

For this purpose it was necessary to operate in *vacuo* with a convenient apparatus;

apparatus; for we have just shewn, that nothing could be proved by experiments of this kind made in air. The following appeared to me to possess every requisite, and I have employed it with success.

§ 33. It is a tube of white glass, two inches in diameter, and 44 inches in length, containing a cylindrical bar of copper, 4 lines in diameter, and 33 inches long, supported in the axis of the tube by wires properly adapted for that purpose, and placed at equal distances from its centre. The two extremities of the bar are hollowed into hemispheres, to receive the bulbs of two very sensible mercurial thermometers; and the tube is adapted to an air pump by one of its extremities, and the vacuum being made, the return of air is prevented by a cock,

Apparatus for ascertaining this fact by experiment.

perfectly air tight, with which it is furnished.

It is then suspended by its extremities in a wooden frame, which is surrounded by a thick pasteboard, designed to defend the tube from the immediate rays of the sun, except in the middle of its length, where the pasteboard is interrupted for about two inches, to give passage to the cone of rays, proceeding from a lens of a foot diameter, and 19 inches focus, made by the famous Parker of London.

From this disposition it is evident that the action of the lens on the middle of the bar will heat it; that this heat will be propagated along it, and will proceed from the centre to the extremities. If, therefore, the fire should mount more readily than descend,

scend, the superior thermometer will be sooner heated, will rise higher, and, *ceteris paribus*, will cool more slowly than the other.

I exposed the apparatus, during half an hour, to the general influence of the solar rays upon the pasteboard, and upon the small portion of the tube left naked, before I began the experiment, in order to disengage the result of the particular influence I sought to discover from this general effect.

I operated in the Observatory at Geneva, a large lofty room, where no accidental heat could modify the result. I was assisted in these experiments by two learned philosophers, who honour me with their friendship, viz. Count Andreani, who was then at Geneva, and Mr. Senebier. One

of us was posted near the superior, another near the inferior thermometer, and a third was employed in directing and preserving the focus of the lens exactly upon the middle of the bar. And we noted the minute and second, in which the mercury reached each division of the two thermometers.

*First exper-*

*iment.* § 34. In the first experiment, a table of which follows, the tube had been exhausted the evening before, until the mercury, in the gage of the air pump, stood at four lines; and I sedulously preserved this degree of rarefaction in all the following experiments. A momentary imperfection in my air pump, did not allow me to render the vacuum more perfect. Previous to the experiment, I tried if any air had entered, and found the vacuum unaltered. The tube was placed

placed vertically, the cock below, and the observations contained in the Table, No. I., give the result of this experiment. The first column to the left indicates the minutes and seconds in which the mercury of the superior thermometer reached the degrees indicated in the second column. The third gives the arrival of the mercury of the inferior thermometer at the same degrees, and the fourth column indicates the difference of time in which the inferior thermometer arrived sooner or later than the superior thermometer at the same degrees of elevation. The sign — placed opposite each number in the column of differences, shews that the inferior thermometer arrived later, and the sign + that it arrived sooner than the superior at the same degree.

Table of the Experiment, No. I.

Sup. Therm.	Degs.	Infer. Therm.	Differences.
3 h. 16' 0''	8.	3 h. 16' 0''	0.''
22.	11.	22.	30.
23.	12.	23.	20.
23.	13.	24.	0.
24.	14.	24.	45.
25.	15.	25.	30.
25.	16.	26.	10.
26.	17.	26.	48.
27.	18.	27.	30.
27.	19.	28.	10.
28.	20.	29.	10.
28.	21.	29.	55.
29.	22.	31.	20.
30.	23.	32.	55.
32.	24.	34.	25.
33.	25.	35.	40.
34.	26.	36.	50.
35.	27.	38.	0.
37.	28.	39.	15.
38.	29.	40.	35.
39.	30.	41.	36.
40.	31.	43.	10.
41.	32.	45.	2.
42.	33.	45.	55.
44.	34.	48.	55.
46.	35.	50.	50.
48.	36.	54.	45.
49.	37.		
51.	38.		
54.	39.		

Mean difference, excluding the last,

101''

§ 35. On the inspection of this table, <sup>1st. Result.</sup> we see, that the superior thermometer has made a more accelerated progress than the inferior, which gradually increased, so that in the same interval of time, viz. in 38', 45", the duration of this experiment, the former rose 31 degrees, whereas the latter was only raised  $28^{\circ}$  by the same calorific cause, acting at an equal distance from each.

If we cast our eyes upon the column of differences, we see, that they augment gradually, although their progress be irregular. I impute this irregularity partly to the inaccuracy of the division of the instruments, and partly to the difficulties attending observations of this kind, without affirming, however, that there might not be some other causes. Be that as it may,

may, the mean difference, excluding the last as necessarily inexact, on account of the slow movement of the mercury in the neighbourhood of the extremes, is found to be 101 sec., by which the inferior thermometer arrived at the same degree of elevation later than the superior. A very considerable difference, and which surpasses what I durst have previously supposed.

However, we shall presently see that this was not entirely owing to the influence I endeavoured to ascertain.

The progress of refrigeration. § 36. I suppressed the action of the lens at the moment the last observation of the foregoing table was made, and observed the progress of refrigeration, which is sufficiently remarkable.

## Cooling.

Sup. Therm.	Degrees	Infer. Therm.	Degrees
b. min. sec.		b. min. sec.	
3. 54. 45.	39.	3. 54. 45.	36, 0.
3. 59. 45.	39, 8.	59. 45.	36, 6.
4. 2. 20.	39, 0.	4. 2. 15.	36, 0.
3. 0.	38, 0.	3. 15.	35, 0.
4. 20.	37, 0.	4. 30.	34, 0.

It is worthy of remark, that the two thermometers continued to rise, although the heating cause had ceased to act upon the middle of the rod; and that at this temperature, the thermometers lost about one degree in 50 sec. by the simple effect of refrigeration. Hence it appears probable, that at this epoch, the mass of fire accumulated in the bar of metal, and which tended to abandon the different parts of it according to a certain law, entered

entered the thermometers more abundantly than it quitted them by the effect of refrigeration. The superior thermometer rose 0,8 and the inferior 0,6 after the suppression of the lens. We shall hereafter find observations of the same kind, and similar results.

*and experi-  
ment.* § 37. My first experiment gave me only presumptions in favour of an *ascensive* movement natural to fire. It was necessary to repeat it with the tube in an inverted position, so that the thermometer, which had been superior in the first experiment, should be inferior in the second; by which disposition it is evident, that the advantages which one of the instruments might have had over the other in the former case, whether by a more perfect contact between its bulb and the bar,

bar, or by a greater degree of sensibility, would be retained by it in the inverse situation, except, indeed, those which depended only on its being situated above the other; and the influence of this position forms the object of my inquiries.

The experiment was repeated, therefore, the following day, at the same hour, with similar precautions, and with the addition of two other thermometers, placed on the outside of the glass tube, near the extremities, to see if the exterior difference of temperature from below, upwards, would be considerable. The vacuum was the same as in the evening before, and the cock perfectly air tight.

Table of the Experiment, No. II.

Therm. exter & superior.	Therm. sup.	Degrees.	Therm. inf.	Differen- ces.	Therm. exter & inferior.
7, 5	b. min. sec.		b. min. sec.	sec.	
	3. 16. 20.	7. 5.	3. 16. 30.	0.	7, 0
	20. 25.	8.	20. 25.	0.	
	23. 2.	10.	23. 0.	+	2.
	24. 5.	11.	23. 50.	+	15.
	24. 48.	12.	24. 45.	+	3.
	25. 45.	13.	25. 30.	+	15.
	26. 35.	14.			
	27. 35.	15.	27. 10.	+	25.
	28. 40.	16.	28. 15.	+	25.
8, 0	29. 55.	17.	29. 10.	+	45.
	30. 50.	18.	30. 15.	+	35.
	31. 45.	19.	31. 10.	+	35.
	32. 45.	20.	32. 0.	+	45.
	33. 45.	21.	32. 50.	+	55.
	35. 20.	22.	34. 15.	+	65.
	36. 47.	23.	35. 40.	+	67.
	38. 24.	24.	37. 10.	+	74.
	39. 48.	25.	38. 15.	+	93.
	41. 15.	26.	39. 55.	+	80.
8, 5	42. 17.	27.	41. 0.	+	77.
	44. 35.	28.	43. 20.	+	75.
	46. 50.	29.	45. 45.	+	65.

Mean difference,

+ 47.

§. 38. On inspecting the column of differences, between the two thermometers in this experiment, we see, that they are in the opposite sense, viz. the inferior thermometer arrived at the same degrees sooner than the superior; but the absolute quantity of this difference is much less. The mean quantity by which the inferior thermometer marked the presence of the same degree of heat sooner than the superior, amounts only to 47 sec. ; whereas, in the former experiment, this mean quantity amounted to 101 sec. If, therefore, we subtract the smaller number from the greater, we shall destroy thereby the individual influence of the thermometers, and the remainder 54 sec. will represent the result of their respective positions, or the mean quantity by which, *ceteris paribus*, the

superior thermometer indicated, more quickly than the other, the same degrees of elevation.

It may be remarked, that the exterior thermometers did not differ considerably from each other, although one was placed at the upper, the other at the lower end of the tube; and that the sun's heat being more feeble in the second experiment than in the first, it raised the thermometer within to only 30 deg.

3d experi-  
ment.

§ 39. A repetition of these experiments being necessary, it was my wish, before I proceeded upon them, to discover some mode of lessening, if possible, the effect produced by the more or less perfect contact of the bulbs of the thermometers in the cavities they occupied at each extremity of

the

the rod. For this purpose I enclosed both ends of it, and consequently the bulb of each thermometer in a slip of oiled paper, which was passed twice round without touching the thermometers. This disposition (on account of the slow conductive faculty of the oiled paper) was calculated to retain around the bulb a part of the heat transmitted along the bar, and perhaps to diminish the inequality resulting from a less perfect contact.

I introduced, on this occasion, two additional thermometers into the tube, whose bulbs were placed opposite the ends of the bar, at the distance of about an inch. These thermometers were observed from time to time during the course of the experiment, and I have included these observa-

tions in the following table. The tube was exhausted to the same degree, as in the preceding experiments, its position the same as in the first, that is, the cock below, and we proceeded in every respect as before.

Table

Table of the Experiment, No. III.

Superior insulated therm.	Therm. sup.	Degs.	Therm. inf.	Differences	Interior insulated therm.
	b. min. sec.		b. min. sec.	sec.	
		8.	3. 15. 0.	0.	8, 0,
		8. +	3. 15. 45.	0.	the ther
	3. 18. 0.	9.	3. 18. 0.	0.	begin to mount.
	3. 20. 20.	10.	20. 20.	0.	8, 1.
	21. 5.	11.	21. 25.	20.	
	22. 25.	12.	22. 38.	13.	
	23. 20.	13.	23. 35.	15.	
10, 2.	24. 38.	14, 2.	24. 45.	7.	8, 8.
	25. 35.	15.	25. 48.	13.	
	26. 5.	16.	26. 45.	40.	
	27. 12.	17.	27. 53.	41.	
	28. 16.	18.	28. 58.	42.	
	29. 13.	19.	29. 50.	37.	9, 4.
11, 1.	30. 10.	20.	31. 0.	50.	
	31. 0.	21.	32. 12.	62.	
	32. 16.	22.	33. 25.	69.	
	33. 10.	23.	34. 40.	90.	10, 1.
	34. 32.	24.	35. 48.	76.	
	35. 43.	25.	37. 2.	99.	
12, 3.	36. 40.	26.	38. 25.	105.	
	37. 50.	27.	39. 55.	125.	10, 8.
	39. 10.	28.	42. 55.	225.	
	40. 35.	29.	46. 5.	330.	
	45. 0.	30.	48. 45.	225.	
	47. 15.	31.	50. 45.	215.	
13, 7.	49. 30.	32.	53. 20.	240.	12.
	52. 0.	33.	4. 1. 45.	585.	12, 2.
	55. 32.	34.			
	4. 2. 2.	35.			
	2. 40.	remove the lens		therm. begins	
	4. 6. 15.		4. 5. 40.	to descend	
Mean, omitting the last.					93 <sup>11</sup>

Results  
conform-  
able to the  
preceding  
experi-  
ments.

§ 40. We have here an experiment made under the same circumstances as the first, and it offers us similar consequences. The mean of 23 observations, omitting the last as too near the extreme, gives 93 sec., as the quantity by which the inferior thermometer marks the presence of the same degree of heat more slowly than the superior.

It is also observable, that the latter rose 35 deg. and the former only 33.<sup>o</sup> in the same interval of time. The difference between the thermometers insulated within the tube was in the same direction, the superior being constantly higher than the other, at every correspondent epocha.

It is remarkable, that being so near the ends of the metallic rod, they should participate so little of its temperature;

perature; for they differed from it towards the end of the experiment, about 21 deg. It appears that the heat communicated itself, with difficulty, through the vacuum of the tube.

§ 41. In this experiment, the time, Remark  
which con-  
firms them. which elapsed after the removal of the lens, until the moment in which each thermometer began, sensibly, to descend, was observed with particular attention, and this interval, as is seen in the table, was 3 min. for the inferior thermometer, and for the superior, 3 min. 35 sec. This difference is in favour of, and tends to confirm the former observations.

At the beginning of the experiment, we endeavoured to ascertain the lapse of time between the application of the focus of the lens to the middle

middle of the bar, and the first sign of motion in the mercury of the inferior thermometer. This interval appeared to be 1 min. 45 sec.

4th experi-  
ment.

§ 42. We had now to repeat a fourth time the same experiment, but in the inverse position, that is, with the cock uppermost, as in the second experiment. We proceeded with the same precaution, and in circumstances similar to those of the aforesaid experiments, and it offered the following results.

Table

Table of the Experiment, No. IV.

Superior insulated therm.	Sup. therm.	Degs.	Inf. therm.	Differ- ences.	Inferior insulated therm.
	<i>h. min. sec.</i>		<i>h. min. sec.</i>		
8, 0.	3. 8. 0.	6, 3.	3. 8. 0.		
	12. 35.	7.	12. 35.	0.	
	14. 10.	8.	13. 58.	+ 12.	
8, 0.	15. 0.	9.	14. 50.	+ 10.	7, 2.
	10.				
	16. 35.	11.	16. 20.	+ 15.	
	17. 20.	12.	16. 55.	+ 25.	
	18. 5.	13.			
8, 5.	18. 45.	14.	18. 14.	+ 31.	
		15.	18. 53.		
		16.	19. 30.		
	20. 45.	17.	20. 10.	+ 35.	
	21. 25.	18.	20. 45.	+ 40.	
	22. 2.	19.	21. 25.	+ 37.	
		20.	22. 2.		8, 5.
	23. 30.	21.			
	24. 18.	22.	23. 35.	+ 43.	
	24. 58.	23.	24. 25.	+ 33.	
	25. 43.	24.	25. 13.	+ 30.	
10, 2.	26. 40.	25.	25. 58.	+ 42.	
	27. 50.	26.	26. 52.	+ 58.	
	29. 0.	27.	28. 0.	+ 60.	
	31. 0.	28.	29. 52.	+ 68.	
	32. 55.	29.	31. 40.	+ 75.	
	34. 30.	30.	33. 37.	+ 53.	
12, 0.	36. 30.	31.	35. 47.	+ 43.	
	38. 10.	32.	37. 30.	+ 40.	
	40. 0.	33.	39. 28.	+ 32.	
	41. 40.	34.	41. 15.	+ 25.	
	43. 55.	35.	42. 38.	+ 77.	
	49. 30.	36.	44. 15.	+ 295.	12, 1.
		37.	47. 0.		
	49. 30.	remove the lens			

The mean of 22 observations, from 8°

to 35°, omitting the last, - - - - + 40"

Result analogous to the former. § 43. We see here a progression similar to that observed in the second experiment. The mean of 22 observations, from  $8^{\circ}$ . to  $35^{\circ}$ . omitting the last, as too near the extreme, gives  $+40$  sec. by which the inferior thermometer indicated the presence of the same degree of fire sooner than the superior: if we subtract these  $+40''$  from  $-93''$  given in the third experiment, we shall have  $53''$  as the mean quantity, by which the superior thermometer would have exhibited the same degree of heat sooner than the inferior, if the only difference between them had been their relative position. This, therefore, is the immediate effect of the natural ascensive tendency of fire. It is surprizing, and can only be the effect of chance, that this conclusive number, the comparison

son of the mean numbers of the two last experiments, should differ only one second from the mean numbers given by the comparison of the two former. The insulated thermometers confirm these results, the superior having been constantly higher than the other, at similar periods of time. In other respects their progress was pretty conformable to that observed in the foregoing experiments.

§ 44. Finally, the observations during the refrigeration, which were made with more care, and carried farther in this than in the three preceding experiments, seem likewise to shew an ascensive disposition in the fiery element; as may be concluded from the following table.

Cooling.

## Cooling.

Superior insulated Therm.	Sup. therm.	Degrees.	Infer. therm.	Degrees.	Inferior insulated therm.
13, 7.	4. 50.	30.	36, 2.	4. 50.	30. 37. 0.
	51.	50.	36, 2.	55.	38. 36.
	54.	40.	36, 0.	57.	30. 35.
	56.	47.	35.	58.	30. 34.
13, 5.	57.	58.	34.	59.	35. 33.
	59.	16.	33.	4.	0. 35. 32.
	4.	0.	35.	1.	46. 31.
	1.	46.	31.	2.	55. 30.
13, 0.	2.	55.	30.	4.	15. 29.
	4.	5.	29.	5.	23. 28.
	5.	20.	28.	6.	32. 27.
	6.	42.	27.	7.	32. 26.
12, 4.	8.	0.	26.	9.	2. 25.
	9.	28.	25.	10.	20. 24.
	10.	55.	24.	12.	20. 23.
	12.	20.	23.	14.	10. 22.
	14.	10.	22.		

§ 45. The examination of this table affords the following remarks:

The superior thermometer rose  $0^{\circ},2$  during the first minute after the removal of the heating cause.

The same thermometer remained afterwards stationary during 80 sec. notwithstanding the cold it necessarily sustained, and which, without doubt, was exactly counter-balanced by the heat it received from the bar. Lastly, at the expiration of 5 min. it was descended only  $0^{\circ},2$  from its point of departure.

The inferior thermometer, on the contrary, was at  $37^{\circ},1$  at the moment the lens was removed. It did not rise: and at the expiration of one minute it was descended  $0^{\circ},1$ .

In 6 min. 8 sec. it descended  $1^{\circ},1$  whereas the other in the same interval

interval of time, to judge from its progress, would not have descended  $0^{\circ}, 3.$

Finally, in 22 min. 50 sec. to reckon from the common point of departure, the inferior thermometer descended  $14^{\circ}, 1$  and the superior  $13^{\circ}, 2$  although the latter having risen higher, and having been more distant from the point of equilibrium, ought to have cooled more quickly than the other.

Conclu-  
sions.

§ 46. It appears that we may justly conclude from these facts, which accord with each other, that fire actually moves more readily from below upwards, than in the opposite direction, which is common to all gravitating bodies; or perhaps that it is lighter than another aerial fluid, in which it swims; or finally, that it is essentially

essentially light. For I cannot presume that the imperfection of the vacuum in my apparatus, which I have said wanted 4 lines, or  $\frac{1}{8}$  part of being perfect, could explain such signal disproportions. To have ascertained the fact, I should have repeated these experiments with the tube full of air, and have seen the influence of this fluid; but the season, already far advanced, and likewise very unfavourable, would not permit the execution of my design. I shall repeat them assuredly, and I invite philosophers to endeavour to verify this important fact, by which many phenomena might be explained.

§ 47. Since these inquiries were made, I have found that the academicians, *del Cimento*, had undertaken

The academy *del Cimento* had tried these experiments.

similar investigations, in an apparatus somewhat resembling mine. They introduced into a glass tube, in which they afterwards made the torricellian vacuum, or vacuum by means of mercury, two thermometers pretty near each other. They heated this tube from without by two hot balls, which, they say, were advanced *a little nearer* the inferior thermometer, in order to counter-balance the effect of the heated air, which might ascend along the outside of the tube, and communicate more heat above than below. They assure us, that having repeated this experiment several times, the superior thermometer appeared always to receive more heat than the inferior. And they add, that the experiment being repeated with the tube

full

full of air, the effect was nearly the same as in the vacuum \*.

Their method of operating was perceptibly very inaccurate. But if their procedure was precisely the same, the tube being empty, and full of air alternately, the trifling difference which obtained in these two cases, proves sufficiently, that the presence of the small quantity of air, which might have remained in the tube in my experiments, could not be sufficient to account for the facts they indicate, without admitting fire to possess a natural ascensive tendency. I remarked during these experiments, that notwithstanding the great heat which the bar of copper sustained in the part

\* Virum tamen est differentiam esse admodum exiguum, posito tubo aeris pleno vel vacuo.

Tentamin. Exp. natur. p. 73, in 4to.

exposed to the lenticular focus, none of those colours were observed, which are produced upon the surface of copper and other metals, when exposed in the open air to an equal degree of heat. These colours, without doubt, depend upon an incipient combination of the metal with the ambient air, which cannot take place when the air is excluded. It would be curious to vary experiments under this point of view, by exposing different metals to the focus of a lens in various aeriform fluids.

C H A P.

## C H A P. III.

*Various Experiments on Heat—Description of the Apparatus employed—Effect of the Colour and Nature of Surfaces relative to their Reflection of Heat—Experiment on the refraction of Heat—Its Velocity—Apparent Reflection of Cold.*

§ 48. FIRE being propagated according to the laws and modifications we have just shewn, traverses the air, and partly unites with it. It may likewise meet solid bodies, in which case a part of the fire penetrates them, and there undergoes the modifications of

Heat is sus-  
ceptible of  
reflection.

which we have already spoken ; another part, of which we are now going to treat, is reflected by their surfaces.

Lambert had already observed, that what he called *obscure heat*, or *heat without light*, was susceptible of reflection. And I place the first ideas of the experiments which I have made on this subject, and which have also given rise to a part of those contained in this Essay, amongst the obligations I owe to my celebrated friend, Mr. De Saussure. I shall here repeat, at some length, the experiment quoted in his *Voyages dans les Alps*, tom. II., p. 353, § 926, ed. in 4to.

Experi-  
ment on  
this subject.

§ 49. We placed two concave mirrors of tin, which form part of my physical apparatus, opposite each other in a large room, at the distance of 12 feet, 2 inches. These mirrors are a foot

a foot in diameter, and their convexity that of a sphere whose radius is 9 inches, and they are but moderately polished.

At the focus of one of these specula, was placed a mercurial thermometer, whose bulb was insulated, and at the focus of the other an iron bullet about 2 inches diameter, heated so as not to be luminous or visible in the dark.

The presence of this bullet raised the thermometer, placed at the opposite focus, from  $4^{\circ}$ . to  $14^{\circ},5$  in six minutes; it stopped there, and began to descend in proportion as the bullet cooled.

§ 50. Let us reflect a moment upon the advantages of this disposition for augmenting the effect of the heat at a given distance. From the position of the mirrors opposite each other,

Reflections  
on the ad-  
vantages of  
the disposi-  
tion of the  
instru-  
ments in  
this experi-  
ment.

G 4 and

and from known catoptric laws, it follows, that a reflexible emanation, excited at the focus of one of these mirrors, is sent back in part by the surface of the mirror in form of a bundle of parallel rays upon the opposite mirror; thence, by a second reflection, it is again collected at the focus of this last, in a degree of density it could not have attained without this contrivance.

Taking the distance of the focus from the surface of the mirror, and the extent of this surface, I compute that, in effect, this mirror receives  $\frac{318}{7000}$  of the calorific emanation produced at its focus, that is, near a third of the whole. It certainly does not reflect this quantity entire to the opposite mirror; a part, without doubt, is again lost by the second reflection, which

which concentrates this emanation upon the thermometer at the focus of the second mirror. I suppose that this double reflection may destroy the half, or if you please,  $\frac{3}{4}$  of it, yet there will remain  $\frac{1}{2}$  of the total emanation thrown by this contrivance to the given distance; whereas, without this arrangement, and by placing the thermometer simply at the same distance from the calorific focus, in such a manner as that it may receive solely a direct emanation, only a part of it would arrive, which part would be to the whole emanation, as the surface of the section of the bulb of the thermometer is to the surface of the sphere, whose radius will be the distance of the thermometer from the centre of the emanation. Thus, supposing, as in our experiment, the

thermo-

thermometer to be 11 feet, 5 inches, from the heated body, and the diameter of the bulb of the thermometer 3 lines, we shall find that the surface of the section of this bulb is only  $\frac{1}{4834027}$  part of the surface of a sphere whose radius is 11 feet, 5 inches, and that the thermometer receives directly, at this distance, only a part of the calorific emanation expressed by this fraction.

The experiment repeated with a wax taper.

§ 51. By placing a lighted wax taper at the focus of the first mirror, instead of the bullet, the thermometer, at the focus of the second, rose in one of our experiments, from  $4^{\circ},6$  to  $14^{\circ},9$  and in another from  $4^{\circ},2$  to  $14^{\circ},3$ . It appears, therefore, that the effect of a burning taper approaches that of a bullet of the diameter

meter and degree of heat I have mentioned.

§ 52. But in the action of the bullet there was only pure heat without light ; and in that of the taper a mixture of light and heat.

Method of separating the action of light from the action of heat.

We thought of separating, to a certain degree, these two causes, by interposing midway between the mirrors a very transparent plate of glass. The transparency of this substance would easily permit the light to pass, and being difficultly permeable to heat, would effectually obstruct its passage ; and, in fact, the presence of the burning taper at one focus having caused the thermometer, placed at the other, to rise from  $2^{\circ}$  to  $12^{\circ}$  where it appeared stationary, we interposed the plate of glass, and in the space of 9 minutes, the thermometer

meter descended to  $5^{\circ}, 7$  which is more than half the number of degrees it had risen without the interpolation of the glass. The plate being removed, the thermometer rose again in 7 minutes to  $11^{\circ}, 1$ . However, the light reflected upon the thermometer by the mirrors, did not seem sensibly diminished by the presence of the glass; it boulted itself in some manner through this transparent body, and thus became separated from the heat, the greatest part of which remained behind.

The same  
experiment  
with a ma-  
trass of  
boiling wa-  
ter.

§ 53. I had still, however, some scruples concerning the experiment with the bullet. I feared that, perfectly obscure as it appeared to us, it might yet be luminous to organs more sensible to the action of light than ours. I believed I should be more

more sure of employing a hot body not luminous, by substituting a glass matraff of nearly the same diameter, containing 2 ounces, 3 drachms of boiling water, which was supported at the focus of one of the mirrors by a little basket of iron wire.

The mirrors in this experiment were 10 feet, 6 inches distant, and I had placed a very thick screen of double silk between them. At the focus of one was a small thermometer of mercury, made by Ramsden at London, adapted to Fahrenheit's scale. The matraff of boiling water being placed at the focus of the opposite mirror, the screen was removed, and in two minutes the thermometer rose from  $77^{\circ}$  to  $50.0\frac{1}{8}$  of this scale; but at the instant the matraff was taken away, the thermometer descended.

Heat is ab-  
sorbed by  
black bo-  
dies.

§ 54. More convinced of the reflexivity of pure heat, I was desirous of seeing if it also possessed, in common with light, the quality of being absorbed by black substances. With this design, I blackened the bulb of the same thermometer, and repeated the experiment in every other respect in the same manner. The thermometer appeared more quickly affected than when the bulb was clean and polished, and the reflected heat produced a more considerable effect, for the mercury rose from  $51^{\circ}\frac{1}{8}$  to  $55^{\circ}\frac{1}{4}$ . I have since pursued the same experiments with a different apparatus, as I shall presently relate.

The use of  
thermome-  
ters of air  
in these ex-  
periments.

§ 55. The matras of boiling water had this advantage over hot bodies, that it afforded an absolute quantity of heat in my experiments, which was always

always the same. But, on the other hand, this heat being inconsiderable, I foresaw that to employ it with success, I ought to counterbalance this less intensity by the use of a more sensible thermometer. Thermometers of air are known to possess a supreme degree of sensibility, but I was prejudiced against these instruments on account of their barometrical effect. Yet I thought I could employ them to advantage in these experiments, in which differences and comparative observations, made within a short interval, were the objects of consideration ; and indeed they have answered beyond my hopes.

§ 56. The construction of these Their construction. thermometers consists in blowing a bulb of 3 or 4 lines diameter, and as thin as possible, on the end of a glass tube

tube 8 or 10 inches long, and of the caliber usually employed for mercurial thermometers of the larger size. Both the bulb and tube ought to be very clean and dry within. The bulb being held betwixt the finger and thumb, the air within is rarified by their heat, and expelled in a convenient quantity : I then plunge the end of the tube into coloured spirit of wine, and remove my fingers from the bulb ; in an instant the air within cools, and the pressure of the atmosphere, being no longer in equilibrium, causes the coloured liquor to mount in the tube. When the column has obtained about 3 lines in height, I remove the tube, and quickly wipe the end of it. The little cylinder of liquor rises in the tube, and stops nearer or farther from its bulb, according

cording as the fingers have more or less rarified the air it contained.

I afterwards adapt to the tube an arbitrary division, graduated by lines, and to render it comparable with the ordinary scale at the temperature in which I make my experiments, I suspend the thermometer of air, near a very sensible thermometer of mercury, in a room of a certain temperature, and observe with a glass (for it is impossible to approach without making them instantly vary) the correspondent degrees on the two scales : I repeat this experiment in another room, whose temperature differs some degrees, and hence I obtain the elements of a comparative division, from which I form a table for each instrument. The extent of their variations depends upon the difference of capacity be-

tween the bulb and the tube. The most convenient are those in which a space of two inches, which consequently admits a very considerable subdivision, answers to one degree of the ordinary scale. These extemporeaneous thermometers are very convenient in all experiments in which there is occasion to observe very small differences of heat.

It is necessary to empty them after they have been used; and for this purpose it is sufficient to heat the bulb a little, and wipe the end of the tube when the liquor is expelled. Without this precaution, if they are exposed to a certain degree of cold, the liquor mounts into the bulb, and the instrument is spoiled; for one cannot afterwards, without much time and patience, and without making the bulb

bulb and tube red hot, dry the interior sufficiently to make use of it for a new experiment.

I place the thermometer at the focus of the mirror, the bulb uppermost. The tube enters freely a small cylindrical support, which serves as a stand, and which may be lowered or heightened at pleasure. The division is marked upon a paper, which scarce exceeds the diameter of the tube, and is pasted upon it; thus the body of the instrument intercepts but a very small part of the rays of heat, which arrive at the mirror, from whence they are reflected to the bulb of the thermometer.

§ 57. I have just said that I had examined, by an apparatus differing from the blackened thermometer, the influence of surfaces of different kinds

in reflecting and absorbing heat. This inquiry makes the object of the following experiments.

I arranged the mirrors at the distance of 50 inches from each other. At the focus of the one was placed the matras of boiling water, and the thermometer of air at the focus of the other.

I here inform my readers, that I generally found the focus of heat of these mirrors by a preliminary experiment on the focus of light. I placed a burning taper in the little basket which the matras was to occupy, and when the thermometer, at the focus of the opposite mirror, presented upon its bulb the distinct, full, and equally luminous *spectrum* of the mirror which reflected the light, I was sure that this light came to it from every point of

of the surface of that mirror, and consequently, that the bulb was at its focus. Having found this point, I left the stand on the spot, in order to complete my thermometer, which required only three or four minutes, and replacing the latter, after having removed the taper, I was sure that it was at the focus. But to return to our experiment :

I procured a plain looking glass, larger than the mirrors already mentioned, and of the thinnest glass I could meet with ; and having mounted it on a ball and socket, like a graphometer, I placed it vertically in the middle between the two mirrors.

This looking glass presented two substances in perfect contact ; the glass, whose two surfaces were equal, and a very thin amalgam, the sur-

face whereof, in contact with the glass, was polished like ordinary mirrors, and the posterior surface exhibited the dead white of tin amalgamated with mercury.

The looking glass being placed so as to intercept the calorific emanation in its passage from one mirror to the other, by making half a turn on its stand, I could present, at pleasure, towards that side from whence the calorific emanation proceeded, that is, towards the matrass, either the polished and brilliant surface of the glass, or the dead white of the amalgam. The difference of their effect upon the thermometer will shew which of these two positions *reflected* or *absorbed* the heat more effectually. It is clear, that in this particular case, in which the intermediate substance remained

remained the same, these two expressions must be synonymous, seeing that the quantity *absorbed* by this substance ought to be the same; and that the quantity *not transmitted* ought consequently to be *reflected*.

§ 58. The polished side of the glass Result. being turned towards the matrass, the mean ascension of the thermometer of air was only 0,5 of a degree. Each of these degrees was equal to  $\frac{1}{24}$  of a degree of the common thermometer.

The amalgamated surface or the back of the glass being turned towards the matrass, the mean ascension was  $3^{\circ},5.$  deg. Hence we see that of two white surfaces, that which is polished reflects heat more efficaciously than the other. I afterwards blackened with indian ink and a little smoke,

the back of the looking glass, and repeated the same experiment.

The polished side of the glass being turned towards the matraff, the ascension was  $3^{\circ}$  deg., and with the back of the glass turned towards the matraff, it was  $9^{\circ},2$ .

We have here a very remarkable difference of the transmission of heat, and this difference of  $6^{\circ},2$  is in favour of that position in which the blackened surface of the glass was directed towards the matraff.

Remark.

§ 59. It will be observed, without doubt, that the effect of the matraff on the thermometer was greater in this last experiment than in the former, made in similar circumstances; for, the polished side of the glass being turned towards the matraff, the thermometer rose, in the first experiment,

ment, to only 0,5 and in the second to 3 deg. The thermometer having been placed more exactly at the focus in the second experiment than in the first, might have occasioned this difference. But as the experiments, which have been the objects of comparison, were made whilst the thermometer remained in the same position, this difference can in no wise change the result of the experiments, which seems to manifest the influence of the nature and colour of surfaces in the reflection and transmission of heat.

§ 60. I was desirous to know to what degree the amalgam and stratum of black contributed to intercept the heat; and therefore, immediately after the aforesaid experiments, I removed all the amalgam of the looking glass, and

Another experiment on the effect of the glass alone.

and began the same experiment. The thermometer now ascended to 18 deg. that is about 9 deg. higher than it had risen in the former experiment, the amalgamated and blackened side being turned towards the matress; and it exceeded 15 deg. the ascension which took place, when the polished surface of the looking glass was directed to that side, from whence the emanation proceeded.

The glass removed.

§ 61. The transparent glass still powerfully intercepted the heat; for when it was removed, or when it was turned only a quarter of a revolution upon its stand, so that its plane was parallel to the course of the emanation, it rose so rapidly, that if the apparatus had been left so disposed, the liquor would have escaped from the thermometer in a few seconds.

§ 62. I then

§ 62. I then substituted a thin piece of white pasteboard of the same dimensions as the glass, in order to compare their effects. The thermometer rose 10 deg. that is, the pasteboard produced nearly the same effect as the glass when covered by the amalgam and blackened, the black part being turned towards the matraffs.

§ 63. Now, as fire possesses the property of being reflected by polished surfaces, and by the same laws as light, it is not impossible but that it may be likewise refracted in similar circumstances.

To ascertain this fact I placed, at the focus of one of the spheric mirrors, the matraffs of boiling water, and receiving the parallel rays reflected by this mirror upon three different lenses, whose thickness at the centre varied

Experiment with  
a piece of  
white paste-  
board.

Trial of the  
refraction  
of heat.

varied from 6 lines to 2,7 and whose focus differed from 24 inches to 67,5 I could obtain no sure sign of greater heat at the focus of any one of them than elsewhere. However, I am far from affirming that heat is not susceptible of refraction : perhaps, as glass transmits it with difficulty, it may be necessary to repeat this experiment with metallic lenses, which afford it a more easy passage.

<sup>The velocity of heat in the air.</sup> § 64. The velocity of heat through the air, when in a state of liberty, has not yet, to my knowledge, occupied the philosopher ; and I have been naturally led to that kind of experiments which seems to determine it. Heat is generally supposed to move somewhat slowly through bodies\*. but I

\* De Luc. Idées sur la Météorologie, § 178.

have reason to presume, from my own experiments, that in this respect we are mistaken, or rather that it is necessary to distinguish, as we shall presently see.

I employed, in this pursuit, the methods pointed out by Mr. de Saussure \*. I disposed two concave mirrors, of a different quality, at the distance of 69 feet from each other. The mirror, at whose focus I purposed placing a heated body, was one of the tin specula already described ; the other was a speculum of gilt plaster of 18 inches diameter, and 15 inches focus. I adjusted my thermometer of air at the focus of the last. This arrangement seemed preferable, because, at so great a distance, the im-

\* Voyage dans les Alpes, § 927.

perfection of the first mirror causes a diffusion of the reflected rays, by which a considerable quantity would be lost if they were not received upon a surface proportionably larger.

A few inches from the focus of the first speculum was placed a very thick screen. The observer then approaches the thermometer, and the experiment does not commence until the heat, issuing from his body, has produced its full effect on the thermometer, and the instrument remains perfectly stationary. The bullet, of which we have already spoken, heated to a certain degree, but not sufficiently to become luminous, is then placed in the basket of iron wire at the focus of the first mirror. At the instant the screen is removed, the thermometer rises, and it is impossible, at the distance of

of 69 feet, to perceive a sensible interval of time between the cause and the effect. But let us suppose that even a second or two might elapse, and the interval is in no case greater between the removal of the screen, and the rise of the thermometer ; this delay might unquestionably be ascribed to the difficult permeability of the glass of the thermometer to the matter of fire, and its velocity will still be so great, that at 69 feet, it will be undeterminable by this experiment, which I have not repeated at a greater distance.

At the moment the screen is replaced the thermometer descends, and mounts again as soon as it is taken away. It even appears that the instrument exhibits, with still greater promptness, the presence of the emanation,

nation, when, by a repetition of the experiment, the space between the focus of heat and the thermometer is, in some measure, filled by the emanation, than when the screen is removed for the first time.

**Reflections.** § 65. If this experiment contradicts received ideas, it likewise contradicts, in appearance, some facts : for, it seems, when we light a fire in an apartment, that the thermometer hung at the other end of the room is a considerable time before it indicates the presence of heat. But if we reflect on the advantageous disposition of the mirrors, which I have explained, § 34, for increasing the calorific emanation at a given distance, compared with the feeble effect of the direct rays, and if we attentively consider the extreme sensibility of the thermometer, the facts

facts I announce may be reconciled to general observation.

§ 66. It appears then, that fire, <sup>Fire moves</sup> <sub>in two ways</sub> whether in the air, or in other bodies, moves, at the same time, in two ways. That part of the calorific emanation which, in traversing bodies, meets only pores and nothing solid, moves in a straight line, and in every direction with a considerable velocity, perhaps as rapidly as sound, or even light; that part, which meets in its passage the molecules of bodies, unites itself with them under the modification of specific heat, and propagates itself more or less slowly, according to the conductive faculty of these substances. The first may be called *radiant heat*, the second *propagated heat*.

§ 67. When I thought of substituting on the side next the thermo-  
meter, <sup>Glass mirrors reflect heat very imperfectly</sup>

meter, in the experiment made at the distance of 69 feet, a large speculum instead of the small one of tin, in order to collect in a large quantity the emanation reflected irregularly, at this distance, by the first mirror, it was not the one of gilt plaster, which I first employed. I imagined that a large concave glass mirror of 16 inches diameter, and 27 inches focus, would best answer my purpose. But I saw, with surprize, that the thermometer, at its focus, indicated little or no heat, notwithstanding the bullet was at the focus of the opposite mirror. However, I presently found out the reason, which was very simple. We have already seen that heat traverses glass with difficulty, and that a considerable portion of it is intercepted in its passage.

stage. In the glass specula it is not their anterior surface that reflects the greatest part of the rays, but more especially the metallic surface applied to the back of the glass. The heat having the whole thickness of the glass to traverse before it arrives at this surface, cannot be reflected without repassing it, and being thus doubly sifted, by a substance which, with difficulty, admits its passage, but little of it escapes to act upon the thermometer. I had recourse, therefore, to a surface purely metallic, in order to avoid this considerable loss, and the speculum of gilt plaster, imperfect as it was, in comparison with that of glass, for the reflection of light, proved much superior to it for the reflection of heat.

What becomes of the fire, which is neither reflected nor transmitted by glass mirrors ?

§ 68. But what becomes of the heat thus intercepted ? We have just seen that it is not thrown by reflection from the mirror, and likewise, by the experiments made with the looking glass, § 42, we are assured that it is not transmitted ; it remains, therefore, in the glass and heats it. It is diffused in the substance of the glass in proportion to its specific heat, and we should perceive its effects, without doubt, if the mirror remained long exposed to the calorific focus.

Reflection  
not cold.

§ 69. I conversed on this subject with Mr. Bertrand, a celebrated professor of Mathematics in our academy, and pupil of the immortal Euler \*. He asked me if I believed cold sus-

\* He is author of a work entitled *Développement de la Partie Élémentaire des Mathématiques*.  
ceptible

ceptible of being reflected ? I confidently replied no ; that cold was only privation of heat, and that a negative could not be reflected. He requested me, however, to try the experiment, and he assisted me in it \*.

I disposed the apparatus exactly as in the experiment for the reflection of heat, and employed the two mirrors of tin, at the distance of  $10\frac{1}{2}$  feet from each other. At the focus of one was a thermometer of air, which was observed with the necessary precautions, and at the focus of the other a matras full of snow.

\* *The academicians, del Cimento,* have endeavoured to concentrate cold at the focus of a concave mirror. But they acknowledge that their experiments had been made in a manner not sufficiently exact to authorize any conclusions.

At the instant the matraffs was placed for experiment, the thermometer at the opposite focus descended several degrees, and remounted as soon as the matraffs was removed.

Having replaced the matraffs at the focus, and thus made the thermometer descend to a certain degree, where it remained stationary, I poured some nitrous acid upon the snow, and the cold thus produced caused the thermometer to descend instantly 5 or 6 deg. lower.

Only apparent. § 70. The fact was notorious, and amazed me at first; a moment's reflection, however, explained it. This phenomenon offered nothing more than a final proof, if it had been necessary, of the reflection of heat—I conceive the matter thus:

Suppose

Suppose A and B, two mirrors, placed in a room of a certain temperature; let us place, at the focus of the mirror A, a thermometer of the temperature of the chamber, and let us conceive the focus of the mirror B to be occupied, as it really is, by a small quantity of the air of the chamber of the same degree of temperature.

We have seen, § 8, that every heated body is, in some measure, in a forced state. Now the thermometer in this experiment, how little soever its temperature may be raised, must be considered as a heated body relatively to every colder body; its fire, therefore, will tend to abandon it, and if it could, would diffuse itself around the thermometer in the form of a radiant emanation, a consi-

derable part of which would strike the mirror A, at the focus of which the thermometer is placed, and would be reflected in parallel rays to the mirror B, from whence it would be conveyed at its focus. But this effect is only *potential*, because the air at the focus of the mirror B, being supposed of the same degree of temperature as the thermometer, the fire it contains will develop the same tension as the fire in the thermometer at the opposite focus; and will resist its escape with a force precisely equivalent to that which the fire in the thermometer will exert to arrive at the focus of B. In this case, then, every thing is in equilibrium, and the fire cannot move, because the resistance is on all sides equal to the tension.

But

But if, instead of the heated air, we should place at the focus of B, a body not only colder than the thermometer which is at the focus of A, but which, like snow or ice, should be of such a nature as to destroy entirely the tension of fire, which arrives there, then the course of the fire of the thermometer, which, in the supposition we have just made, was only *potential*, would become *actual*. The presence of the cold body would open a kind of gulph to the heat of the chamber; it would absorb the more powerfully the heat of the thermometer, as it would arrive by a kind of funnel, because the mirror A would receive (§ 34) about a third of the calorific emanation proceeding from the thermometer, and would reflect it to the mirror B, from whence it would be

be entirely lost in the snow contained by the matraſs at the focus. This arrangement is extremely advantageous for depriving the thermometer of its heat, in comparison with the direct effect of the same quantity of snow at an equal distance. We have here the same advantage in absorbing the heat of the thermometer, that we had in increasing it, when instead of a colder we placed a hotter body at the focus B. Hence the experiment made with snow does not, in fact, otherwise differ from that made with the bullet, or the matraſs of boiling water, than by the direction in which the calorific emanation moves. In the latter it moves from the bullet or matraſs of boiling water to the thermometer; from the focus of B to the focus of A; and in the experiment made

made with snow, it moves in a contrary direction, viz. from the thermometer to the matrass of snow; consequently the thermometer acts the same part relatively to the snow as the bullet in relation to the thermometer.

§ 71. And this explication will apply, although we should look upon the calorific effect as resulting, not from a real emanation, but from a peculiar vibration in fire considered as an elastic fluid, filling the space in which the experiment was made. It is known that these vibrations are susceptible of being reflected according to the same laws as emanations, of which the reflection of sounds affords us daily examples.

## C H A P. IV.

*Description of the Apparatus made use of for observing the Transmission of Heat through some elastic Fluids.*

Method of proceeding. § 72. THE examination of the phenomena, which might appear in the transmission of heat through various elastic fluids, and even through a vacuum, presented a very extensive and interesting field of experiments; and those of which I have just given an account, afforded me the means of proceeding in this investigation after a new mode that might prove advantageous.

By

By the method I pursued it was easy for me to heat a thermometer in a certain transparent medium, without heating (considerably, at least, in comparison with the thermometer) the medium itself, which, moreover, I could vary at pleasure. By observing the progress of the heating and cooling of the thermometer in these various circumstances, their differences might lead to conclusions more or less important, which, to me, would certainly be new. The description of the apparatus I made use of to accomplish these designs, will be the subject of the present chapter.

§ 73. T, fig. I., represents the thermometer of mercury, whose bulb is  $2\frac{1}{2}$  lines in diameter; not being mounted, the divisions are marked upon the tube itself, and extend from .5 deg.

5 deg. below freezing point to the 40th degree above, which is marked near the top of the tube; the tube itself is formed into a ring at its extremity, and is hung upon the stan.-l SS, which is also a glass tube; a pair of small forceps is fixt to this tube, destined to keep the thermometer in its place and prevent its oscillations. From this disposition, and by employing bodies difficultly permeable to heat, it is evident I sought to avoid the dissipation of it when once arrived at the thermometer.

The elec-  
trome ter.

§ 74. Towards the top of the glass tube, SS, is a small electrometer E, composed of a piece of metal in form of a T reversed, fixt to this tube by a ring, and bearing, at each extremity, two pair of little balls, suspended by very fine metallic threads, like the electro-

electrometers of Mr. De Saussure. One pair of these balls is made of the pith of the elder tree, the other of two turnip seeds. These seeds being heavier than the pith of the elder tree, afford less sensible indication, and serve, by that means, to graduate, in some measure, the symptoms of electricity, by diverging with less facility.

§ 75. HH is an hygrometer of <sup>The hygro-</sup> hair, differing only in the form of its <sup>meter.</sup> mounting, from that invented by Mr. De Saussure. C the hair fixt at the upper part, and whose inferior extremity occupies the bottom of a semicircular groove, or kind of half pulley, of the same piece with the index of the instrument, and moving with it upon a common pivot. H; the scale divided into 100 parts, from the

the term of extreme dryness to extreme humidity at the upper part, which answers to 100. I shall not enlarge upon the qualities of this instrument, which have been profoundly developed in the *Essais sur l'Hygrometrie* of the learned inventor, and as the instrument itself is pretty generally known to philosophers. But independent of the advantages which render this instrument, in general, valuable, it possesses one quality in a supreme degree, that makes it inestimable in the kind of experiments I had undertaken. I mean its sensibility, or the promptitude with which it conforms to the hygrometric state of the surrounding medium. In this particular it surpasses the thermometer itself, as we shall see hereafter.

§ 76. R represents a small gage or <sup>The gage or manometer.</sup> little barometer in form of a syphon, intended to indicate either the degree of vacuum, obtained in the receiver in which certain experiments were made, or the manometric effects of the elastic vapours introduced into the vacuum. That branch of the syphon on the side R is open at the top ; the other closed. The mercury contained in the tube had been accurately purged of air by ebullition ; and the division shews the state of rarefaction from the degree where the air is capable of supporting 3 inches of mercury down to the level, if it were possible to attain it.

§ 77. These four instruments are introduced into the globe BB, fig. I., and at fig. II. they are represented in miniature. The neck of this globe <sup>General dis-  
position of  
the appara-  
tus.</sup> K is

is covered by a brass cap, and luted with the lute described by Mr. De Saussure\*, which is very convenient in every case of this kind.

The whole apparatus, represented by fig. II., may appear complicated at first view ; because various objects, connected with different experiments, and which are not all employed at the same time, are brought together in this figure.

Its general disposition shews the means employed to heat the thermometer placed in the centre of the globe, and at the same time to heat, but very little, the fluid by which it is filled. It is by means of two wax tapers and the mirrors arranged as is represented in the figure.

\* *Essais sur l'Hygrometrie*, § 83.

We know, that when in a spheric mirror the radial point is placed somewhere along the axis between the focus of the mirror and the center of the sphere, of which the mirror makes a part, the reflected rays converge towards a certain point along the same axis, and there form, by their union, the spectrum of the radial point. The course of some of these rays is traced on one side of the figure, and it will be understood, without saying any thing more, that, by this disposition, the thermometer will be heated independent of the surrounding medium. In order to find the true position of the tapers, I employ the means indicated § 41, and raise them on their stands in proportion as they consume.

Capacity of  
the globe.

§ 78. The ballon or globe is of thin white glass in the form of a pear, and contains 777 ounces, 6 drachms, 2 scruples of water, at 10 deg. temperature, which gives for its capacity 1200,199 cubic inches, from which deduct 3,908 for the volume occupied by the various instruments, and there will remain 1196,291 cubic inches, for the volume of fluids subject to experiment within it. The neck, with its brass cap, is inserted into a wooden ring open at one side, and supported by three brackets mortised into a circle which serves as their base. The globe thus mounted, and the two wax tapers are placed upon a board, open in the middle, and this board is supported by a tripod with an open top.

§ 79. Two pipes screwed into the metallic cap, which covers the neck of the ballon, communicate with its <sup>Monome-  
trical apparatus applied to the</sup> globe. interior. One of them descends vertically, and is furnished with a cock at R, by which it may be opened or shut at pleasure; a little lower is a second cock, whose key may be seen in the figure. This pipe unscrews between these two cocks, and terminates in a cylindrical glass receiver, open below, and divided into parts, which are thousandths of the total capacity of the globe. This receiver is introduced into a second (C) open above to receive a quantity of water, which mounts to its level in the interior vessel; the inner receiver is then screwed upon the ballon, at the place already mentioned, between the two cocks.

Hence, supposing the two cocks open, and the passage from the globe to the interior receiver free, it is easily understood, that if the air contained in the globe should be dilated, a part of it will pass into the interior receiver, which bears the division just mentioned, and pressing the water it contains, will raise it in the same proportion in the exterior vessel, and the difference of level will become a very sensible index of the least change of volume in the air of the ballon.

But to know the real augmentation of volume produced by the dilating cause under a similar pression, I lower the exterior receiver C, by means of a screw on the tripod T, which supports it, in proportion as the volume of the air augments, and as the level of the water tends to fall in the interior,

rior, and to rise in the exterior receiver, and I thus maintain the water at its level N, in the two vessels. So that the divisions of the interior vessel, seen through the exterior, will indicate very exactly the augmentation of volume of the air within the globe, the pression remaining the same; that is, the air in the globe being constantly in equilibrium with that of the atmosphere. It is understood that we must reverse the rule when the air of the ballon is condensed or diminishes in volume.

§ 80. Finally, the lateral pipe, use of the lateral pipe. which is also furnished with a cock r, occasionally communicates with various pieces, and amongst others, with a long glafs tube in form of a syphon of unequal branches, mounted on a slip of deal EE, divided into inches

and lines, as is represented in the plate. A quantity of mercury sufficient to rise in the two branches to  $n$ , may be introduced, and it may be then adapted to the lateral pipe  $l$ . This part of the apparatus is designed to shew, by the variation of the level of the mercury, and its ascension in the long branch of the syphon, the condensation of an air introduced into the globe. If, for example, the difference of level in the two branches should equal the height of the barometer, at the moment of the experiment, the air in the ballon would then be compressed by a weight equal to that of two atmospheres, and its density would be double.

Exactitude  
of this ap-  
paratus. § 81. The whole of this apparatus, some parts of which are of a very difficult and delicate construction, was executed

executed by Mr. Paul, an artist of great estimation with the natural philosophers of this city, and of uncommon address and sagacity. His reputation is too well established to need my approbation, but I cannot deny myself the pleasure of attesting that the cocks, which are often a defective part of instruments of this kind, are so perfectly air tight, that having made a vacuum in the ballon to a line and a half of the gage, I found no sensible difference at the expiration of six months, although the air might have entered the two cocks, R and r, if they had not been perfectly tight.

## C H A P. V.

*Preliminary Experiments — Effect of the Light of burning Tapers reflected upon a blackened Thermometer — Influence of Daylight — Examination of the Effect of the sides of the Globe upon the calorific Emanation of the Wax Tapers, and of the mean Heat of the air within during the mean Continuance of these Experiments — The Advantages of this Apparatus for manometrical Experiments.*

On the em- § 82. **T**HE experiments related in  
ployment of tapers as the third chapter had shewn me that  
a calorific cause. heat passes through glass with extreme  
difficulty,

difficulty, and I found, that in order to reverberate, by means of the apparatus just described, a degree of heat somewhat considerable upon the thermometer shut up in the middle of the globe, a matras of boiling water placed at the focus of each mirror would be insufficient, and that more active, and, at the same time, the most constant causes of heat possible (such as burning tapers, for example) would be requisite.

As yet we have had no good explication of the manner in which a strong light sometimes excites heat ; but the fact is not, therefore, the less certain, and I think we may consider the taper, at the focus of each mirror, as a body at once hot and luminous, which, by the reflection of this double emanation, produces upon the thermome-  
ter

ter a greater effect than if it had been a hot body not luminous, or than if it had diffused light without heat.

Their effect upon a blackened thermometer.

§ 83. We know that black bodies powerfully absorb luminous emanations, and that from thence heat results. We have likewise seen, § 57, that the same bodies are, as we may say, transparent to heat, that is, they transmit it readily. I was therefore curious to try upon the same thermometer, alternately blackened and bright, and suspended in the open air at the common focus of the two mirrors, the effect of these two causes united in the tapers.

I blackened, at the flame of a candle, the bulb of a thermometer to such a degree, that the image of a lighted candle was still reflected by the bulb itself.

The thermometer thus prepared and adjusted at the common focus of the two mirrors, rose in 1025 seconds, from 8 degrees to 34, which I found to be the *maximum* of the calorific effect of the tapers.

The same instrument perfectly cleaned and bright, placed in the same manner, rose in 937 seconds from 8 degrees to 22, where it also reached its *maximum*. By comparing the time in which an equal ascension had taken place in these two examples, I found that, with the bulb clear and bright, it had required 713 seconds to raise the thermometer from 9 degrees to 21; and that to produce the same effect with the bulb blackened, it had required only 260 seconds.

Thus

Thus we see, that the bulb of the thermometer blackened, receives, from the double action of the light and heat of the tapers, a calorific influence much surpassing that which takes place when the bulb is perfectly bright and clean.

Effect of the  
black stra-  
tum on the  
cooling of  
the ther-  
mometer.

§ 84. Having compared the effect of heat upon the same thermometer, whose bulb was alternately bright and blackened, I was desirous of knowing the influence of the same circumstances in its *refrigeration*.

The result is very remarkable, as may be seen by comparing the numbers of the following table. The first column expresses the degrees of the thermometer ; the second, the refrigeration from degree to degree, the bulb being clean ; the third, the time of refrigeration with the bulb blackened.

Degrees of the therm.	Intervals of the refrigeration from degree to degree in seconds.	
	Bright bulb.	Black bulb.
11.	97. "	113. '
12.	86.	100.
13.	68.	76.
14.	50.	68.
15.	46.	51.
16.	41.	48.
17.	34.	39.
18.	36.	40.
19.	24.	30.
20.	26.	30.
21.	508.	595. "

The

The refrigeration appears slower, the bulb being black, than in the other case, and on comparing the sum of their differences, it will be found nearly in the proportion of 5 to 6. The cause of this difference is probably the resistance made by the black covering to the passage of the heat, when it tends to quit the thermometer where it was collected; for we know that charcoal is one of the worst conductors of heat.

Daylight  
has some  
influence  
on the  
blackened  
bulb.

§ 85. The remarkable influence of the black stratum in augmenting the calorific effect of the tapers upon the thermometer, led me to suspect that daylight alone might raise the blackened thermometer higher than one which was clean.

In order to verify this suspicion, I chose two thermometers of mercury, which

which agreed well with each other. I blackened the bulb of one of them, and then placed them together in a dark closet. At the moment I opened the door I found them agree, but the action of the light soon raised the black thermometer 2 or 3 tenths of a degree above the other, and this difference continued as long as they were mutually exposed to daylight, and ceased as soon as they were again shut up in darkness. I am persuaded that the same cause produces a similar effect on the thermometers of spirit of wine, whose colour is a deep red, and that if they agree with those of mercury at night, they are raised a little more in daylight, and so much the more as the light is stronger. I have several times thought I observed this variation; and it is known that this

L difference

difference amounts to 10 degrees and more, when exposed immediately to the rays of the sun.

By placing at the centre of the balloon, designed for these experiments, a blackened thermometer, I should, without doubt, have raised its temperature: but the effect produced upon the clean and polished bulb was sufficient for my purpose; the presence of the stratum of black would have complicated the results; and its influence in certain experiments, which I meditated, would have been hurtful: I therefore preferred making use of the thermometer described at § 57, without any addition.

Examination of the effect of the sides of the balloon on the calorific emanation of the tapers.

§ 86. Having found the calorific effect of the two tapers upon the thermometer suspended in the open air at the common focus of the two mirrors, placed

placed in the manner described at § 61, it became necessary, before I commenced a course of experiments in the ballon, to examine to what degree its sides intercepted the calorific emanation. Two consecutive experiments were to be made for this purpose, in which every circumstance should be absolutely the same, except that in one the thermometer was placed at the common focus of the two mirrors within the globe, and in the other the same instrument at the same place in the open air, the globe being removed. The comparison of the heating and cooling of the thermometer in these two cases would give me the effect of the sides of the globe.

Here follows the table of these experiments.

Heating.			Cooling.		
Degrees	Therm. in the ballon.	Therm. without the ballon	Degrees	Therm. in the ballon.	Therm. without the ballon
9.	88."	33."	21.	...	26."
10.	85.	38.	20.	...	24.
11.	92.	38.	19.	...	36.
12.	100.	48.	18.	...	34.
13.	138.	54.	17.	67."	41.
14.	130.	52.	16.	83.	46.
15.	160.	66.	15.	103.	50.
16.	330.	80.	14.	99.	68.
17.	...	91.	13.	196.	86.
18.	...	77.	12.	185.	97.
19.	...	52.	11.	257.	
20.	...	84.	10.	372.	
21.	...	198.	9.		
Sum - -		1123'	911"		

It is observable on the first inspection of this table, that the movement of the mercury exhibits more irregularity in the process of heating than during the refrigeration; and the whole course of these experiments confirms this remark, the cause whereof it is not difficult to ascertain. I saw that very inconsiderable differences in the placing of the tapers, in the vivacity of their flame, had a very sudden and notable influence on their calorific effect. The observations from degree to degree made during the cooling, which were affected by no other causes of irregularity than the combined errors of the division of the thermometer, and of the observations themselves, ought, for this reason, to exhibit a more regular progression; and for this reason, also, I

give more faith in general to the results of the observations made during the refrigeration, than to those drawn from the observations made during the heating of the thermometer.

We see also, that the thermometer, placed within the globe, rose, by the action of the tapers, from  $9^{\circ}$ . to  $17^{\circ}$ . in the space of 1123 seconds; and without the globe, in circumstances otherwise perfectly the same, it rose in 911 seconds from  $9^{\circ}$ . to  $22^{\circ}$ . that is, five degrees higher.

By comparing the intervals of time, during which a similar rise had been produced in these two circumstances, I found that the thermometer without had risen in 409 seconds the  $8^{\circ}$  which, within the globe, it had required 1123 seconds to attain.

This consideration shews us already, that the sides of the ballon intercepted a quantity of heat nearly equal to  $\frac{2}{3}$  of the whole emanation reflected by the mirrors.

And if we attend to the cooling of the thermometer in the open air, and within the ballon, we shall see, that it required 733 seconds to descend from  $17^{\circ}$  to  $11^{\circ}$  within the globe, and only 388 seconds to descend the same number of degrees in the open air; that is to say, the refrigeration was effected nearly one half quicker in the latter case.

If, therefore, the thermometer had been heated in the open air by a third part of the heat, which passed through the sides of the globe in the experiment of heating, the refrigerant action of the open air would have destroyed

the effect of almost half this quantity upon the thermometer; hence we may conclude, that the sides of the globe intercept about  $\frac{1}{6}$  of the calorific emanation, which would otherwise arrive at the thermometer. And although, in order to simplify the calculation, which is only an approximation, I have taken for granted that the *periods of time*, during which the same increase of temperature is produced upon the same body, are proportionate to the intensity of the heating cause, which, perhaps, may not be rigorously true, particularly in the extremes; yet, I believe, the error resulting therefrom, in this particular case, will not merit attention.

Enquiry in-  
to the mean  
heat of the  
air in the  
globe.

§ 87. The first enquiry to be made after these preliminary experiments on the effect of the sides of the globe, was

was the mean degree of heat produced in the air of the globe, by the process I used to heat the thermometer. Two causes tended to heat this air. 1st, The heat *propagated* through it; 2nd, the heat accumulated in the thermometer again issuing from it, and diffusing itself through this medium. Thermometers placed within at different parts of the globe would have indicated this mean temperature; but they would have complicated the apparatus, and a method more simple presented itself; viz. the *manometric* effect of heat upon this air, or the augmentation of volume it underwent during the experiment.

In the description of my apparatus, it has been shewn by what means I could observe this dilatation within much less than a thousandth part of the

the total volume; and in order to deduce from hence the heat of the air, not having made any direct experiments upon this subject, I could avail myself of the principles of other philosophers in this respect. And amongst those who have examined this subject in particular, General Roy appeared to me to merit great confidence. It results from the experiments given in his excellent memoir concerning barometrical measurements, inserted in the Philosophical Transactions of the Royal Society of London for the year 1777, that in a temperature between  $52^{\circ}$  and  $62^{\circ}$  of Farenheit (or  $+ 8 \frac{5}{9}$  and  $+ 13 \frac{3}{9}$  of the scale of 80 parts) the mean expansion of atmospheric air is 0,0026 of its volume for each degree of increased temperature. This result applied to the scale of 80 parts,  
the

the degrees of which are to those of Farenheit, as  $2^{\circ} \frac{1}{4}$  to 1. gives 0,00535 of dilatation for each degree of this scale \*.

Now the mean dilatation of the air in three experiments, whose mean deviation

\* Mr. Trembley, in his Memoir on Barometric Measurements, which concludes the 2nd volume of *Voyages dans les Alpes de Mr. de Saussure*, adopts a proportion somewhat different, viz.  $\frac{1}{192}$  or 0,00521 augmentation of volume of common air for each degree of the scale of 80 parts. This number is, without doubt, the nearest the truth, if we consider it with Mr. Trembley as the mean coefficient of results affected by a crowd of different elements, and above all by the errors necessarily committed in the estimation of the temperature of the air, as I shall elsewhere shew (see chap. viii.). But this proportion of dilatation did not appear to merit a preference, in this particular case, to that resulting from the direct experiment

deviation was 32 minutes, was found to be 0,01490, which, according to the estimation already mentioned, gives  $2^{\circ},5$  for the mean heat of the air in the globe during the experiments, in which the mean ascension of the thermometer, placed in the middle of this mass of air, was  $11^{\circ},8$  the point of departure being between 7 and 8 degrees.

Advantages of this apparatus for manometric experiments. § 88. The great advantage of this apparatus for a course of manometric observations is manifest. We are enabled by its means to mark with the greatest exactitude the dilatations

periment of General Roy, against which I see no objection, so long as he has remained within the degrees of heat considerably below those which convert water into an elastic vapour capable of displacing atmospheric air.

corres-

corresponding to the changes of temperature to which the whole of the air it contains is subjected. The hygrometer would always shew the state of the air under experiment, relatively to the watery vapours, and the apparatus, by its nature, would constantly preserve the same degree of pressure on the air shut up in it, as the ambient atmospheric air supported. I look upon this last circumstance as very important, and no manometric apparatus known to me possesses it. These experiments will soon become the subject of a work, in which I have been long occupied, directed particularly towards perfecting the measurement of heights by the barometer.

## C. H. A. P. VI.

*Experiments in the dry and moist  
Vacuum filled with the Vapour of  
Ether—With electric Fluid.*

Plan of the first experiments. § 89. PREVIOUS to the investigation of the motion of fire in aeriform fluids, it was necessary to consider it in vacuo, then to make it pass through watery or other elastic vapours introduced into this vacuum. Thus the modifications of fire, observed in the most simple combinations, would become less difficult to distinguish, when I should have occasion to add to these elastic fluids, whose nature is not truly aërial, either atmospheric air or other perma-

permanently elastic fluids, which may all contain more or less of these same vapours. The experiments made in *vacuo*, under this point of view, form the subject of the present chapter.

§ 90. I employed for the exhaustion <sup>Exhaustion of the ballon.</sup> of the ballon two different air pumps.

The largeness of the pipe of the first rendered its operation rapid, but its construction did not allow me to obtain thereby the degree of rarefaction I desired. I employed, therefore, for this purpose, one of the new pumps made by Hunter of London. This instrument, by a particular and very ingenious mechanism, supplies the defect of elasticity in the last portions of air remaining in the receiver, which are unable to raise the valve of the common pumps, and thus remedies, to a certain point, this defect, which hinders

hinders us from obtaining, in these instruments, a very considerable degree of rarefaction. But even by this improved machine I have never been able to procure a vacuum in the globe, beyond the term where the elasticity of the remaining fluid supported the mercury of the gage at 1 line  $\frac{3}{5}$ ; but I attribute at least one half of this effect to the watery elastic vapour which remains in the globe. And to obtain even this degree of vacuum, it was necessary to employ the following means: having gained a certain degree of rarefaction by the ordinary method, I heated the globe at a clear fire, until the thermometer within rose to  $40^{\circ}$ . At this temperature I placed it on the pump, which I worked until the mercury in the gage appeared stationary.

Notwithstanding this degree of refraction, it is certain that some watery elastic vapour still existed in the globe; because the hygrometer never descended lower in these circumstances than the 13th degree above the term of artificial siccity, obtained by means of alkalis, chosen by Mr. De Saussure as the inferior extreme of his hygrometer: besides, it is known that this elastic vapour is inexhaustible, and that it is reciprocally produced and destroyed by the alternate motions of the sucker. However, the vacuum at this degree of the hygrometer may be considered as very dry, compared with the degrees bordering upon the extremes of humidity, in which, by way of opposition, I proposed to make the same experiments.

§ 91. The particulars I am about to give of the experiments made in the dry and moist vacuum, will serve as examples of the manner in which I proceeded in all the others, whose details I shall therefore omit, and only state the comparisons made between them. All these experiments were made in my cabinet of physic, in which I never light a fire. This chamber is 21 feet in length, and 14 in breadth, and the apparatus was placed in the middle. That the combustion of the tapers might be as uniform as possible during the experiment, I allowed them to burn some time before I adjusted them at the focus of the specula. And I remained at the same distance from the apparatus the whole time of the experiments, which were commonly made in an initial temperature of

## State of the instruments.

Before the experiment.				After the experiment.			
Within the balloon	Ther.	Hygr.	Gage.	Ther.	Hygr.	Gage.	
6,2	17.	1,4		8.	17,0	1,4	
Without the balloon	6,8	84.	....	7,6	80.	....	
Heating.				Cooling.			
Difference from one degree to another in h. min. sec.		Degrees of the therm.		h. min. sec.		Difference from one degree to another in seconds.	
99."	2.	31.	51.	7.			
93.	33.	3.	8.	3.	43.	20.	
100.	35.	3.	9.		34.	53.	507."
97.	36.	42.	10.	29.	40.		313.
97.	38.	20.	11.	25.	30.		250.
101.	39.	57.	12.	22.	10.		200.
96.	41.	38.	13.	19.	32.		158.
101.	43.	14.	14.	17.	25.		127.
135.	44.	55.	15.	15.	30.		115.
143.	47.	10.	16.	13.	48.		102.
167.	49.	33.	17.	12.	18.		90.
190.	52.	29.	18.	10.	58.		80.
			Tapers burn ill.				68.
			55.	30.	19.	9.	50.
			147.	57.	57.	20.	65.
			153.	3.	30.	21.	8.
			345.	6.	15.	22.	45.
							70.
							7.
							35.
							maximum.
							Duration of the cooling.
			2064."				2145."

## State of the instruments.

Before the experiment.		After the experiment					
Within the balloon	Without the balloon	Ther.	Hygr.	Ther.	Hygr.	Gage.	
	6,5.	81.	...	7,5.	79.	...	
Heating.		Cooling.		Difference from one degree to another in Hygro- meter.		Difference from one degree to another in Hygro- meter.	
Hygro- meter.	Difference from one degree to another in seconds.	h.	min.	h.	min.	sec.	sec.
93.	86."	2.	29.	8.	7.		
92,5.	74.	30.	34.	8.	3.	35.	50.
	64.	31.	48.	9.	26.	5.	585.
	64.	32.	52.	10.	20.	55.	310.
92,2.	82.	33.	56.	11.	16.	52.	243.
	97.	35.	18.	12.	13.	35.	197.
91,3.	99.	36.	55.	13.	10.	44.	171.
	82.	38.	34.	14.	8.	34.	130.
103.	103.	39.	56.	15.	6.	33.	113.
90,1.	128.	41.	39.	16.	4.	40.	94.
	238.	43.	47.	17.	3.	6.	82.
88,8.	205.	47.	45.	18.	1.	42.	86.
87,5.	140.	51.	10.	19.	3.	0.	16.
	270.	53.	30.	20.	2.	59.	16.
		58.	0.	21.	maximum.		
	1782."	Duration of the heating.		Duration of the cooling.		2194."	

of 6 to 8 degrees. In the following table of the experiment made in the dry vacuum, the middle column shews the degrees of the thermometer; those on each side shew the hours, minutes, and seconds, in which the mercury reached each degree of the division traced upon the tube, as well ascending as descending; and the two exterior columns mark the intervals, from degree to degree, in seconds.

Remarks. § 92. On the inspection of the column of differences in the process of heating, we observe an irregularity, owing to the burning of the tapers, of which I have spoken, § 70; and we also remark that the intervals from 19 to 20, and from 20 to 21, although they approach nearer the *maximum* than the two preceding intervals, have been traversed in less time, because the tapers burnt better.

It may likewise be noticed, that the first interval, whether in the process of heating or of cooling, is a little longer than that which immediately follows. This fact, which appears pretty constantly in the course of my experiments, may be explained by the inertitude of the mercury of the thermometer \*.

\* Muschembroeck had observed it in his pyrometers.

By adding the time elapsed from one degree to another in these two experiments, we see that the thermometer employed 2064 seconds in mounting from 7°. to 22°. where it arrived at its maximum, and 2145 seconds in descending from 21°. to 8°.

But I here inform my reader, that in the comparison I am about to make of the results of various experiments, I shall always exclude the extreme intervals towards the maximum of heat, and towards the last term of refrigeration. The motion of the thermometer being then so slow, as necessarily to render the observation not exact.

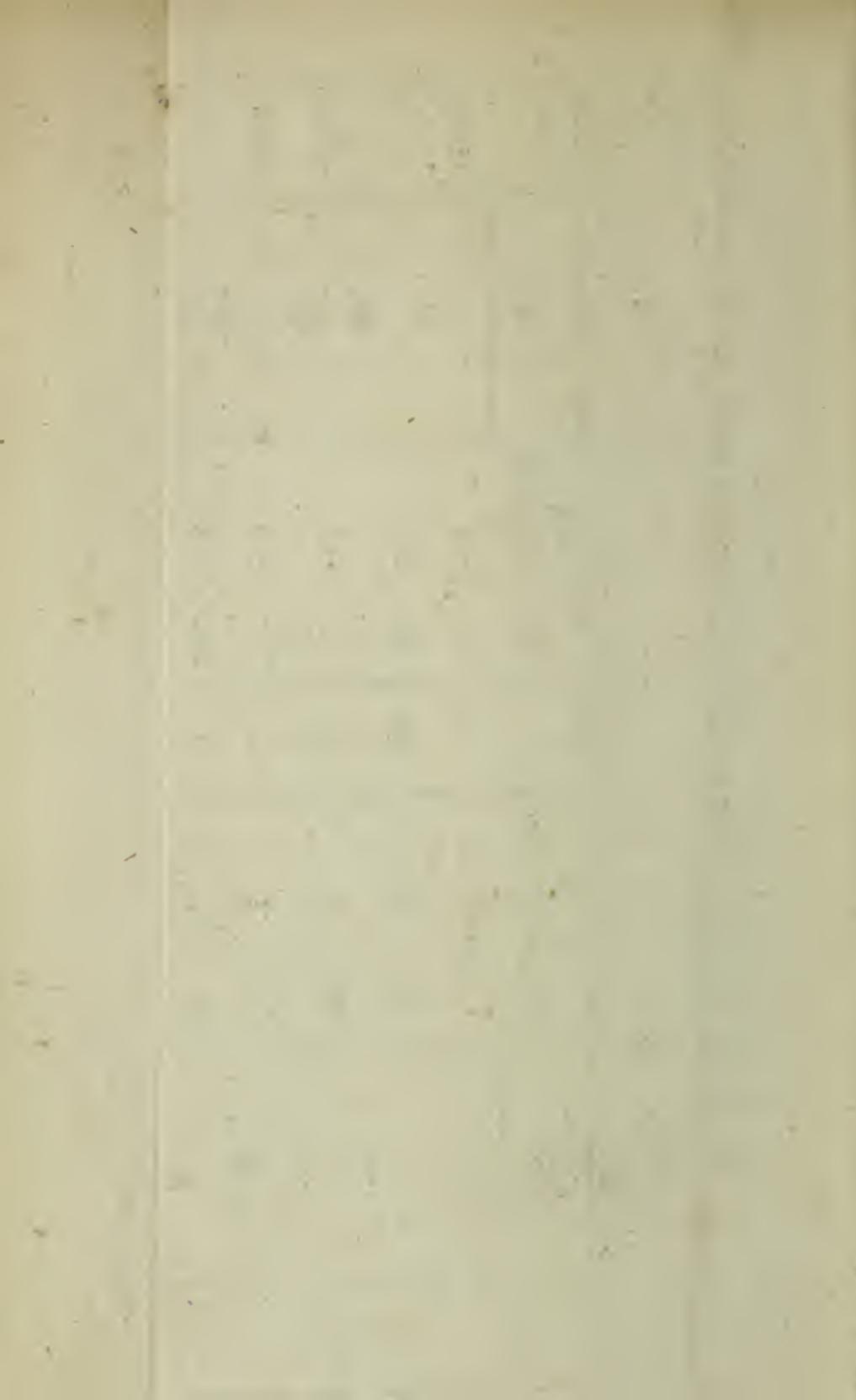
§ 93. In order to repeat the same experiment in a moist vacuum, I waited until the external circumstances were as conformable as possible to those

which obtained during the experiment in the dry vacuum. The tapers and every other part of the apparatus were the same ; the sides and interior of the globe were equally transparent in both experiments. I suppress the particulars, which relate to the introduction of the aqueous vapour, and to some experiments which it occasioned. These, together with the phenomena presented by the ethereal vapour, in similar circumstances, will make the subject of the following chapter.

The table of the experiment in the moist vacuum contains two additional columns, designed for the hygrometer, whose variations in this experiment are worthy notice, although scarcely observable in the former.

### State of the instruments.

Before the experiment.		After the experiment				
Within the ballon	Without the ballon	<i>Ther.</i>	<i>Hygr.</i>	<i>Ther.</i>	<i>Hygr.</i>	<i>Gage.</i>
		6,3.	94.	6,8.	91.	4,5.
	Heating.					
	Difference from one degree to another in seconds.					
93.	86."	2.	29.	8.	7.	Degrees.
		30.	34.	8.	3.	h. min. sec.
92,5.	74.	31.	48.	9.	26.	50.
	64.	32.	52.	10.	20.	5.
92,2.	64.	33.	56.	11.	16.	55.
	82.	35.	18.	12.	13.	243.
91,3.	97.	36.	55.	13.	35.	197.
	99.	38.	34.	14.	10.	171.
82.	103.	39.	56.	15.	8.	130.
	90,1.	41.	39.	16.	34.	121.
89,1.	128.	43.	47.	17.	6.	113.
	238.	47.	45.	18.	3.	88,8.
88,8.	205.	51.	10.	19.	4.	94.
	140.	53.	30.	20.	40.	82.
87,5.	270.	58.	0.	21.	16.	86.
	1782."	Duration of the heating.				60.
						87,8.
						2194."
						Duration of the cooling.



The process of heating exhibits irregularities of the same sort as those we observed in the foregoing experiments, and they are clearly owing to the unequal combustion of the tapers.

§ 94. The difference of moisture in the two experiments we are going to compare, answers to  $76^{\circ}$  of the hygrometer, which was at  $17^{\circ}$  in the former, and at  $93^{\circ}$  in the beginning of the last experiment. The watery vapour introduced into the ballon acted upon the gage with a pression, capable of sustaining a column of mercury of  $3\frac{1}{2}$  lines, at the mean heat of the experiment; for, although no air had been introduced, we observed the gage, in the second experiment, at  $4^{\circ}, 5$ ; whereas it was only at  $1^{\circ}, 4$  in the first.

The progress of the hygrometer is nearly what might have been foreseen from a knowledge of the excellent experiments of Mr. De Saussure, and from what has been said (§ 71) concerning the mean heat of the interior of the globe.

The following table shews the comparison of the two experiments.

Heating.		Degrees.	Cooling.		Total duration. Sum, omitting the last interval.
Dry vacuum.	Moist vacuum.		Dry vacuum.	Moist vacuum.	
99."	86."	7.			
		8.			
93.	74.	9.	507."	585."	
100.	64.	10.	313.	310.	
97.	64.	11.	250.	243.	
97.	82.	12.	200.	197.	
101.	97.	13.	158.	171.	
96.	99.	14.	127.	130.	
101.	82.	15.	115.	121.	
135.	103.	16.	102.	113.	
143.	128.	17.	90.	94.	
167.	238.	18.	80.	84.	
190.	205.	19.	68.	86.	
147.	140.	20.	65.	60.	
153.	270.	21.	70.	...	
345.	...	22.	...		
2064.	1732.		2145.	2194.	
1719.	1462.		1638.	1609.	

In the heat-  
ing.

§ 95. Let us first consider the heating: We observe the *maximum* at  $21^{\circ}$  in the moist, and at  $22^{\circ}$  in the dry vacuum. This result is not surprizing, as it is easily to be conceived, that the watery vapour, disseminated in the globe, ought to intercept part of the *radiant* heat; and besides, as it presents a body whose specific heat is greater than the specific heat of the vacuum, it would absorb more efficaciously the fire of the thermometer as it arrived, and would prevent it from acquiring that degree of tension in the instrument which it would have acquired in a pure vacuum.

The more speedy heating, which took place in the moist vacuum, would seem to contradict this explanation, if we could not reasonably impute the difference to the unequal combustion of

of the tapers, of which we have seen indubitable proofs in the process of heating. And I consider myself the more authorized to attribute it to this cause, as in a former experiment (19th Nov<sup>r</sup>. 1785) made in a dry vacuum, I obtained the same *maximum* of  $22^{\circ}$  in a space of 1700 seconds, or in 1450 seconds, if we omit the interval which preceded the *maximum*. These numbers approach very near their correspondent numbers 1722 and 1462, which I obtained in the moist vacuum; and I should have chosen this experiment for my comparison, if the vacuum had not been less perfect by  $\frac{1}{2}$  line.

§ 96. The duration of the cooling And in the cooling. was 41 seconds, or about  $\frac{1}{38}$  longer in the moist vacuum; and yet two circumstances should have contributed to

to accelerate the refrigeration in this experiment. 1st, The thermometer rose to  $21^{\circ}$ . only, and in the dry vacuum it rose to  $22^{\circ}$ . 2nd, The whole duration of the heating was  $2064''$  in the dry, and only  $1732''$  in the moist vacuum; by which means, the thermometer, the space within the ballon, and the sides of it also, ought to have accumulated much more heat in the first experiment; and yet the cooling of the thermometer was quicker in the dry than in the moist vacuum, which I endeavour to account for in the following manner: This *propagated* heat, which results from the affinity of aggregation between the fire and the watery molecules, and which has so much diminished the *radiant* heat upon the thermometer, seems to me to explain the slowness

slowness of the refrigeration; for the fire disseminated in the vapour, is retained, by virtue of this affinity, with a certain energy; and consequently the rupture of equilibrium between the fire of the thermometer, and that which is diffused in the surrounding medium is so much less, and the cooling of the thermometer necessarily so much slower\*.

### § 97. Having

\* In a course of experiments made by Mr. Benjamin Thompson, on the conductive faculty of a vacuum, and of dry and moist air, (Phil. Transac. 1780, 2nd part) he agrees with me, that a vacuum does not conduct so well as air: but he finds, on the other hand, that moist air conducts better than dry. I was much surprised on reading this remark; but I soon discovered the reason of it, in the very method the author employed in these experiments. His apparatus consisted of a thermometer inclosed in

Experiment  
in the moist  
etheral va-  
cuum.

§ 97. Having thus tried, to a cer-  
tain degree, the influence of watery  
vapours introduced into the vacuum,  
upon

in a globe of glass of a much larger diameter ;  
he plunged the globe alternately into ice and  
boiling water, and observed the time of the  
heating and cooling of the thermometer ; but  
in his experiments on moist air, he humected  
the air by wetting the inside of the globe which  
contained the thermometer “ *surrounded by air*  
“ *rendered as moist as possible by wetting the inside*  
“ *of the cylinder and globe with water.*”

Now it may easily be conceived that this  
water suddenly converted into an elastic fluid  
by the heat of boiling water, is thrown upon  
the thermometer with that energy which it is  
known this mixt fluid is capable of exerting in  
these circumstances, and deposites, in falling  
upon the thermometer, the heat of evaporation  
it had acquired by its contact with the interior  
surface of the glass. The circumstances, there-  
fore, of our experiments are not similar ; but  
whoever

upon the cooling of the thermometer placed in the middle of them, I was desirous of trying the effect of the vapour of vitriolic ether, which may be considered as of an oily nature. The following chapter contains the particulars of the introduction of these vapours, and the phenomena which accompanied it. It will be sufficient to observe here, that I could not obtain a dry and ethereal vacuum at the same time; because the water, which makes an integral part of ether, however well it may be rectified, enters with it into the ballon.

whoever will read Mr. Thompson's excellent memoir, will see, that every time the experiment has been made in degrees of heat much below boiling water, the results agree with mine, that is, that moist air is proved to be not so good a conductor of fire as dry air.

It

It may be observed in the following table, that the progress of heating is somewhat irregular, which may be attributed partly to the defects of the tapers, and partly to imperfect observation. The ethereal vapour liquified and extended the varnish, with which the divisions on the tube of the thermometer were then traced, and rendered it impossible to observe with precision. I even suspect that an error of a minute has occurred between the 11th and 12th degrees of the thermometer. The course of the hygrometer seems to approach that observed in the moist simple vacuum. The height of the gage declares the elasticity of the ethereal vapour, to which its elevation is solely owing, as no air had entered the globe.

## State of the instruments.

Before the experiment.			After the experiment.		
Within the ballon	Ther.	Hygr.	Gage	Ther.	Hygr.
Without the ballon	8,4.	96.	11,5.	9.	96.
8,5.	80.	...	9.	79.	...
Heating.			Cooling.		
Hygro-meter.	Difference from one degree to another in seconds.	Degrees.	Difference from one degree to another in seconds.	Hygro-meter.	
96.	83 "	8,4.			
95.	87.	9.			
94,5.	98.	10.	480."	95.	
94,2.	67.	11.	305.	94.	
93,8.	122.	12.	305.	93,5.	
93,7.	95.	13.	185.	92,9.	
93,3.	85.	14.	165.	92,6.	
93,2.	133.	15.	136.	92,2.	
92,8.	134.	16.	135.	92,1.	
92,5.	176.	17.	111.	91,8.	
92,0.	180.	18.	83.	91,5.	
91,6.	159.	19.	75.	91,4.	
91,5.	206.	20.	90.	61,0.	
91,2.	175.	21.			
91.		21 $\frac{1}{4}$ .	maximum.		
	800."		2070."		
	duration of the heating.		duration of the cooling.		

Compari-  
son of the  
two experi-  
ments in  
the simple  
and ethere-  
al vacuum.

§ 98. We have now to compare two experiments to discover the influence of the ethereal vapour. That of § 77, made in the moist vacuum, the hygrometer at  $93^{\circ}$ , and the last in a vacuum at the same time moist and ethereal. The moisture of the vacuum was somewhat greater in this, than in the former experiment; in other respects, the circumstances were nearly similar. The point of departure was  $7^{\circ}$  in the first, and in the last  $8^{\circ}, 4$ ; but although it commenced at this degree, I have suppressed the first interval to avoid reductions, and I only compare the corresponding degrees.

Heating.		Degrees.	Cooling.		Total duration.
Moist simple vacuum.	Moist ethereal vacuum.		Moist simple vacuum.	Moist ethereal vacuum.	
64."	87."	9.	310."		
64.	98.	10.	243.	480."	
82.	67.	11.	197.	305.	
97.	122.	12.	171.	305.	
99.	95.	13.	130.	185.	
82.	85.	14.	121.	165.	
103.	133.	15.	113.	136.	
128.	134.	16.	94.	135.	
238.	176.	17.	84.	111.	
205.	180.	18.	86.	83.	
140.	159.	19.	60.	75.	
270.	206.	20.	...	90.	
		21.			
1572.	1542.		1609.	2070.	
1302.	1336.		1299.	1590.	Sums, omitting the last interval.

The *maximum* appears the same, within  $\frac{1}{4}$  of a degree in the two experiments. But, in fact, the thermometer rose less in the ethereal vacuum, by reason of the smaller distance from the point of departure, which, we have observed, was about two degrees higher in this than in the former.

The duration of the heating was nearly equal, and we shall see, by the difference in the cooling, that this equality is probably owing to the compensation of two opposite effects. The *radiant* fire certainly arrived with more difficulty through the aeriform ether, than through the simple watery fluid, and for this reason the heating ought to have been slower. But the ethereal vapour, being more difficultly permeable, would retain more efficaciously, in the thermometer, the fire which

which tended to escape, in proportion as it accumulated.

In comparing the refrigerations, we cannot oppose, one by one, the intervals elapsed between similar degrees of the thermometer, because the whole extent is two degrees more considerable in the first than in the second experiment; but by comparing the sums, and omitting, as usual, the last intervals, I find the cooling of the same number of degrees, reckoning from the *maximum*, lasted 1299 seconds in the moist vacuum, and 1590 in the moist ethereal vacuum; that is, it was nearly a quarter flower in this last case.

And yet this is the least possible difference; for if I had considered as the last interval of refrigeration in the moist vacuum, not the number 310.

which expresses the cooling between  $9^{\circ}$  and  $10^{\circ}$ , but the number 585", the duration of the cooling between  $8^{\circ}$  and  $9^{\circ}$ , and truly the last interval; we should have had the comparison of the cooling in the moist vacuum expressed by the numbers 1024 and 1590; that is, the cooling, in this last case, would have been about one half slower than in the simple moist vacuum. This difference is indeed considerable.

It is not surely to the specific heat of the ethereal vapour, that we ought to attribute this enormous difference, but rather to the difficult permeability of this vapour to liberated fire. The absolute quantity of fire introduced into the globe was nearly the same in both experiments, since their total durations only differ 30 seconds, and even

even this difference is in a contrary sense to that which would tend to explain the difference of these results by the quantity of fire introduced. It is probable, therefore, that the fire is not retained in the ethereal vapour by affinity, as the specific heat of ether not being so great as that of water, it is likely that the same proportion subsists in a state of vapour. I presume that the ethereal vapour acts as an oily substance, and we know that these substances are very bad conductors of heat. It is, therefore, from the difficulty which fire experiences in traversing a vapour of this nature, that it so slowly quits the thermometer.

§ 99. After these experiments on <sup>Effect of the</sup> ethereal vapour, I proposed to try, <sup>ethereal va-</sup> <sup>pour on the</sup> <sup>apparatus.</sup> in the same manner, the effect of spi-

rituous vapours, obtained from spirit of wine. But the ethereal vapour had attacked the varnish in every part of the inside of my apparatus, so that it became necessary to suspend my experiments, in order to repair it. The hair of the hygrometer, deeply impregnated by this oily vapour, had lost its sensibility, and I was obliged to substitute another. I feared lest these inconveniences should recur, in some degree, during my intended experiments on spirituous vapours; I therefore renounced them, to examine the electric fluid, which, at the same time, presented me with an attracting novelty.

Apparatus  
for experi-  
ments on  
the electric  
fluid.

§ 100. I am in possession of an excellent electrical machine, whose cylinder, made by the famous Parker of London, is 53 inches in circumference,

ference, and 23 inches long. The glass is very electric, and when it is in order, and the weather favourable, I can draw sparks of some density from the extremity of the conductor, at the distance of 15 inches.

The general disposition of the apparatus was the same as during the preceding experiments; the globe, placed  $2\frac{1}{2}$  feet from the conductor of the electrical machine, was supplied with electric fluid by a metallic communication, connected with the brass ring which surrounded the neck of the globe, and the board that supported the globe and the two tapers, instead of being placed upon the tripod, as in fig. 2, was placed upon an insulated stand. The exhausted globe, thus disposed, would be filled with the electric fluid as long as the experiment

periment of heating and cooling continued.

First trial. § 101. I first examined whether electricity alone would have any effect upon the several instruments contained in the globe. The instruments were as follows: Thermom.  $9^{\circ}, 9$ . Hygrom.  $12^{\circ}, 8$ . Gage 1,75. Electrom. 0. I electrified during five minutes, and saw no movement in any of the instruments. It will, without doubt, be thought surprising, that the electrometer, although very sensible, did not diverge; but if we reflect, that the ballon was filled within, and surrounded without by a dense positive electric atmosphere, and that the instrument plunged in this atmosphere could not be deprived of its natural electricity, our surprize will cease.

§ 102. I now made two consecutive experiments of heating and cooling. The first by continually electrifying the apparatus, and the second without electricity. The instruments previous to these experiments were as follows: Hygrom.  $13^{\circ}$ . Gage 1,85. Thermom. in the globe  $+ 9^{\circ}$ ,. Thermom. in the chamber  $9^{\circ}, 5$ . I omit the experiments themselves, in order to give their comparative results.

Experiments in the electrified and non-electrified vacuum.



§ 103. It is very remarkable, that Remarks. the process of heating was more speedy by nearly one third in the electrified than in the simple vacuum; and this difference is maintained from degree to degree in an unquestionable manner.

This circumstance will appear still more extraordinary, if we consider, that this speedy heating was produced by a calorific cause, whose energy was manifestly weaker; for, in this last experiment, the electricity which escaped by the tapers caused an inconstancy in their flame, and in general they burnt very ill. Hence, although the heating cause acted more speedily, it had less absolute density in the electrified vacuum, where the thermometer rose only to  $17^{\circ}\frac{1}{2}$ , than in

in the simple vacuum, where it rose to  $20^{\circ}$ .

This fact may be explained, by supposing that the electric action produced a certain heat, which, uniting with the calorific emanation of the tapers, augmented their effect to a certain point; but the heat, thus produced, being confined in the extent of its action, could not influence the *maximum*, which was diminished by the imperfect combustion of the tapers.

As to the refrigeration, the observation marked by an asterisk appears to merit rejection. Nor ought we to compare the first degree of this process, from  $17^{\circ}$  to  $16^{\circ}$  in the electrified vacuum, with the corresponding degree of the other series; seeing, that

that the first degree of refrigeration is always slower, from a kind of stagnation which the fire experiences when the course of its direction is changed; finally, we ought to reject, as we have always done, the last interval of the cooling. There will, therefore, remain only three degrees to be compared, which offer nearly a similar progression.

§ 104. These first results led to a repetition with the tapers insulated.

discovery of facts too interesting for me to neglect repeating and varying the experiment which had furnished them. I therefore repeated, as soon as possible, the same experiment, with this difference only, that I now insulated the tapers, by placing them on large glass salvers, varnished with copal. The instruments were as follows:

low: Thermom. + 9° Gage 1,85.  
Thermom. in the chamber 9°,5.

I omit the detail, and give only the result, as in the preceding experiment.

## Insulated Tapers.

Heating				Cooling.			
Deg.	[Interval from degree to degree in seconds.]	Differ- ces.	Deg.	[Interval from degree to degree in seconds.]	Differ- ces.		
	Electrical vacuum.	Simple vacuum.		Electrical vacuum.	Simple vacuum.		
9.	80."	105."	+ 25."	11.	360."	295."	- 65."
10.	120.	95.	- 25.	12.	215.	200.	+ 15.
11.	100.	90.	- 10.	13.	235.	170.	+ 65.
12.	85.	85.	00.	14.	188.	165.	+ 23.
13.	80.	95.	+ 15.	15.	123.	110.	+ 13.
14.	90.	95.	+ 5.	16.	108.	113.	- 5.
15.	105.	80.	- 25.	17.	99.	150.	- 51.
16.	118.	98.	- 20.	18.	82.	82.	00.
17.	122.	87.	- 35.	19.	105.	75.	+ 30.
18.	150.	135.	- 15.	20.	....	73.	
19.	205.	265.	+ 60.	21.	....	57.	
20.	....	190.		22.	....	60.	
21.	....	220.		23.	....	78.	
22.	....	165.		24.	....		
23.	....	274.					
24.	1050."	965."		1410."	1285."		
	Sums from 19° to 9°			Sums from 19° to 11°			

Remarks. § 105. In this experiment with the tapers insulated, the difference in the promptitude of the heating between the electrified and simple vacuum disappeared; but we find a variation in the opposite sense, viz. the thermometer in the simple vacuum traversed the intervals from  $9^{\circ}$ . to  $19^{\circ}$ . more quickly than in the electrified vacuum, in the proportion of 965" to 1050".

The thermometer rose 4 degrees higher in the simple than in the electrified vacuum. This fact was easily foreseen; because, notwithstanding the insulation of the tapers, they were too much affected by the dense electric atmosphere to burn so well during the time the electrical machine was in action, as during the time it was in repose.

The refrigerations, compared in the correspondent intervals, appear more slow in the electrified than in the simple vacuum, in the proportion of 1410" to 1285" viz. about  $\frac{1}{11}$ . Was this occasioned by the electric fluid entering the globe united to the fire, which became its *deferent* fluid, and acquired intensity by that union, or was it a consequence of the difficult permeability of the electric fluid to fire? Besides, the progress of the refrigeration offers striking irregularities, of which I know not the cause, as they even exceed the bounds of imperfection in the observations themselves.

§ 106. To know whether these new <sup>3d</sup> exper-  
results ought to be imputed to the in-  
sulation of the tapers, it was necessary  
to repeat the experiment in the same

ment with  
tapers not  
insulated.

circumstances as in the first essay of this kind, which was accordingly executed, with this difference only, that I was obliged to employ new tapers, the former being consumed. I made two consecutive experiments of heating and cooling; first without electrifying, and then by electrifying continually. The results were as follows: Therm. 9,5 Hygrom. 15°. Gage 1,8.

## Tapers not insulated.

## Heating.

## Cooling.

Deg.	Interval from degree to degree in seconds.		Differ- ces.	Deg	Interval from degree to degree in seconds.		Differ- ces.
	Electrical vacuum.	Simple vacuum.			Electrical vacuum.	Simple vacuum.	
10.	...	112."		12.	254."	255."	+ 1."
11.	...	88.		13.	197.	190.	- 7.
12.	80."	85.	+ 5."	14.	187.	180.	- 7.
13.	100.	93.	- 7.	15.	123.	123.	00.
14.	90.	107.	+ 17.	16.	119.	122.	+ 3.
15.	90.	115.	+ 25.	17.	97.	90.	- 7.
16.	100.	125.	+ 25.	18.	90.	95.	- 5.
17.	65.	107.	+ 42.	19.	81.	95.	+ 14.
18.	80.	123.	+ 43.	20.	89.	75.	- 14.
19.	115.	150.	35.	21.	55.	65.	+ 10.
20.	115.	160.	+ 45.	22.	70.	60.	- 10.
21.	165.	120.	- 45.	23.			
22.	195.	165.	- 30.				
23.	180.	240.	- 60.				
23 <sup>1</sup>							
	1190."	1350."			1362."	1340."	
	sums from 12° to 23°				sums from 12° to 23°		

Remarks.

§ 107. We here find a result of the same kind as in the first experiment, but in a smaller degree. The electric force accelerated the heating in the proportion of 1190." to 1350," omitting the last intervals, which would still increase this difference. The *maximum* was the same in both cases, viz. 23 $\frac{1}{4}$ . It is true, that in the electrical experiment, the temperature of the chamber was one degree higher, which must have influenced the *maximum*. The refrigerations made a progression nearly parallel, and much less irregular than in the experiment, § 87. It may here be observed, that when there are no real causes of irregularity, the uncertainty in the observation is reduced to very narrow limits.

Conjectures  
on this sub-  
ject.

§ 108. If we may be permitted to reason on the results which are offered by

by the six experiments that have been compared: if we may admit what they seem to indicate, viz. that the thermometer is more rapidly heated, when a continual electric force is joined to the action of the uninsulated tapers, and that it cools more slowly in the electrified recipient when the tapers are insulated; perhaps these facts would not remain without a plausible explanation. In the case of the tapers not being insulated, the electric fluid arrives by preference at the flame of the taper, and there forms an emanation; this emanation mingles with the hot and luminous emanations emitted by the tapers, and may be partly reflected with them, considering that the mirrors themselves are plunged in the same electric atmosphere, and are in some degree insulated; probably

the electric fluid itself being *thus* introduced into the globe, may develop heat, or a luminous quality capable of exciting heat.

In the experiment with insulated tapers, their flame does not receive more electric fluid than the rest of the apparatus, nor more than a proportional part of the electric atmosphere which surrounds the globe, and therefore, not being radiated at the focus of the calorific emanation, is not in the course of being reflected to the centre of the ballon.

## C H A P. VII.

*Experiments relating to Evaporation, and to Hygrometry in general.*

§ 109. I HAVE already observed, <sup>Sensibility</sup> that the experiments made in <sup>of the hair</sup> vacuo, <sub>hygrometer</sub> had presented some interesting facts, which I think it now time to particularize. They belong for the most part to hygrometry, and have shewn me the inestimable value of the present Mr. De Saussure has made to natural philosophers in giving them the *hair hygrometer*.

That my readers may form an idea of the advantages of this instrument, I shall begin by relating the experiment

ment I previously made with an intention to try the sensibility of the hygrometer within the globe.

I separated the hygrometer from the rest of the apparatus, and employed a glass cylinder, humected within after the manner of Mr. De Saussure, in order to expose the instrument alternately, and as rapidly as possible, to extreme moisture, and to the dryness of the air in which I operated. I first reduced the hygrometer to 100 or extreme moisture, under the wet cylinder, and then suddenly removing the glass, I observed the following progression, and noted the intervals of time which the needle of the hygrometer required for every 5 degrees.

From

From	100°.	to	95°	4 sec
	95.		90	4 $\frac{1}{2}$
	90		85	8 $\frac{1}{2}$
	85		80	13
	80		75	4°

---

In all from 100 to 75. 70 sec.

It is observable, that the instrument required little more than a minute to pass from extreme moisture to the hygrometric degree of the air of the chamber, which was in fact about 75 degrees. The hygrometer being stationary, I again covered the instrument with the wet cylinder, and observed for every five degrees the following intervals.

From	75°	to	80°	4 sec.
	80		85	4 $\frac{1}{2}$

From

From	85	to	90	$5\frac{1}{2}$
	90		95	8
	95		100	38
				—
In all from	75		100	60 sec.

Thus it appears that the hair is impregnated somewhat more quickly in the atmosphere of extreme moisture, than it is exsiccated in the open and quiet air. But in both cases the transition is very prompt. It is likewise true, that of all the hygrometers which I have employed in my several experiments, this in particular possessed the most eminent degree of sensibility.

Its ordinary progression by simple refrigeration. § 110. The well-directed inquiries of Mr. De Saussure have taught us, that temperature has a direct influence on the degree indicated by the hygrometer; that is, in proportion as a given

given volume of air, containing a certain quantity of water in a state of elastic vapour below the term of saturation, shall be cooled, the hygrometer will move towards humidity, until it arrives at the term of extreme moisture; that it will stop there, although the cooling should continue: but that we shall then instantly observe the water in substance deposited in form of dew upon all the surrounding solid bodies.

Our celebrated author explains this movement of the hygrometer very naturally, by saying that the presence of fire augments the dissolving force of the ambient air, and that its absence diminishes the same, and renders the air less capable of imbibing the moisture from the hair. The relative force of the atmospherical air, and of

the

the hair of the hygrometer to attract the aqueous fluid from each other reciprocally, varies according to certain laws dependant on their respective distances from the term of saturation. The facts, and the theory derived from them, are represented in the most clear and satisfactory manner in the work I have repeatedly quoted.

Remarkable exception.

§ 111. A striking exception to this general law presented itself at my first experiment in *vacuo*. And as it shewed me how active and energetic a part fire acted in evaporation, when unobstructed by the air, it induced me to conclude, that it may, perhaps, be the only agent which produces the phenomena of evaporation, and that the air has little or no share in it. The fact is as follows :

On the third of January, 1786, my globe being exhausted of air, and saturated with watery vapour, so that at the temperature of the chamber, viz.  $4^{\circ}$  above zero, the hygrometer marked  $98^{\circ}$ , that is, extreme moisture; no dew was discernible upon its inner surface. I removed it into another chamber, where the mercury stood 4 degrees lower, exactly at freezing point. It had scarcely been here a minute, before the dew appeared, mixed with some concrete drops, but not frozen. The dew was constantly deposited upon that side of the globe towards the nearest window of the chamber; a circumstance of which we shall presently see the reason.

In this state, who would not suppose that the hygrometer remained stationary at the term of saturation, or extreme

treme moisture ? It was with surprise, therefore, that I saw it move briskly towards siccitv; at the end of 4 minutes it was at  $91^{\circ}$ . only, and the thermometer within the globe was descended one degree. The hygrometer continued to move towards siccitv, and after a few minutes was at  $89^{\circ}$ .

But at the expiration of 20 minutes, the thermometer being descended to zero, I found the hygrometer again mounted to  $94^{\circ}$ . and in 5 minutes more it was at  $97^{\circ}.\frac{1}{2}$  where it remained stationary. Here we see the hygrometer advancing towards *siccitv* in proportion as the watery vapour, in which it was plunged, *cooled*; and we are about to observe an appearance of the same kind in the contrary case, but in an inverse direction.

§ 112. At present, the thermometer in the globe is at zero, the hygrometer at  $97^{\circ}\frac{1}{2}$ , and some dew deposited within upon the side next to the nearest window. I remove the apparatus from this temperature into another chamber, where the thermometer is at  $+ 6^{\circ}$ . and where, consequently, the globe will be heated. At the instant of its arrival, the hygrometer rose to  $99^{\circ},3$ , that is, it moved so much towards moisture, and remained there as long as the evaporation of the dew, which covered a part of the interior of the globe, continued. As soon as this evaporation was completed, the hygrometer began to move towards siccitv, although the bulb of the thermometer, and every part of the apparatus, which occupied the centre of the globe, were covered,

in turn, with the dew. By degrees it disappeared, and at the expiration of three hours, the thermometer was at  $7^{\circ}$ . (the temperature of the chamber having augmented one degree) the hygrometer at  $90^{\circ}$ , and the dew no longer discernible within the globe. In this example, then, the hygrometer begins to move towards moisture, when the globe is heated.

**Explication of these extraordinary appearances.** § 113. I think these extraordinary appearances may be thus explained.

In this globe saturated with watery vapour, we have three substances, viz. water, hair, and fire. Let us consider the water as purely passive; the fire and the hair contend for the aqueous fluid, and the fire, independent of its hygrometric or *cohesive* affinity with water, possesses a locomotive faculty which the hair does not,

not, and by virtue of which it is always in motion towards the place where its tendency to equilibrium invites it ; that is, from hotter to colder : in these movements it carries the water along with it, and, when once it has reduced this liquid to a state of elastic vapour, it is momentarily united with it ; to use the happy expression of Mr. De Luc, it becomes its *deferent* fluid.

When, therefore, in a temperature of + 4, the hygrometer is at 98. and no water in substance is visible in any part of the globe, it shews that both the fire and the hair retain as much water as possible in a state which Mr. De Saussure calls *pure* elastic vapour ; for it is in this state only, viz. when united to the fire of evaporation, that water penetrates the hair in the

true hygrometric modifications\*. The fire is actually in a state of repose and equilibrium, nor tends to move towards any side, because I suppose the temperature within and without the globe to be alike.

But as soon as the whole apparatus is removed into a colder chamber, the equilibrium is destroyed, the fire has a tendency to re-establish it, and

\* Mr. De Luc did not, perhaps, pay sufficient attention to this principle of hygrometry, when he sought the extreme term of his hygrometer in *water*, instead of saturated air; for it is not in a state of *water*, but of *watery elastic vapour*, that this element is united with the air, or with whatever aeriform fluids we apply the hygrometer to; and whenever the air contains water in substance, it either rains or is foggy, and then the hygrometer teaches us nothing that we do not know or see without it.

instantly

instantly moves from the centre to the circumference of the globe ; it quits the hair in particular, and carries with it a part of the elastic watery fluid. In consequence of this sudden departure of the vapour which moistened the hair, the hygrometer moves towards siccitv. The *deferent* fluid which carries away this vapour, both from the hair and from the medium that held it suspended within the globe, penetrates the glass, but not being able to convey the aqueous fluid through this substance, it is necessarily deposited on the interior surface, on all sides indifferently, provided the chamber, to which the globe has been removed, be equally cold in every part. But when it is colder without doors than in the apartment, the vapour will be deposited on that side which happens

to be nearest to the window of the chamber\*. And in general we can direct this appearance of dew to whatever side we please, by the approaching of any cold body. I have several times succeeded in making the hygrometer at the centre of the globe move some degrees towards siccitv by applying a piece of ice to the outside.

But the equilibrium between the hygrometric force of the fire, and of the hair, is soon re-established in this new temperature. The excess of water is deposited in the form of dew,

\* I know not whether the influence that determines the side of the vessel on which certain saline vegetations form, although it may have been imputed to light, might not more justly be attributed to fire operating in a manner analogous to its action in the example above mentioned.

the fire evaporates the rest, and diffuses it uniformly in the interior of the globe; the hair absorbs it in a less absolute, although in an equal relative, quantity, and by degrees it returns to the term of extreme moisture, or very near it.

In these circumstances, let us remove the apparatus from the cold temperature into a hotter, the fire will follow an opposite course. It now tends, from without, inward, and having traversed the glass, meets immediately with the dew that lines the inside, is impregnated with it, becomes its *deferent fluid*, transports it almost instantaneously to the centre of the apparatus, and deposits it upon the instruments which are there, and which itself penetrates. The hygrometer mounts to extreme moisture, if

it was not there already, and remains stationary; because the water in substance, deposited upon the hair in form of vesicular vapour, does not affect it\*. Finally, the quantity of fire which arrives and accumulates in the globe, evaporates successively all the water it encounters; we no longer perceive any dew, and the hygrometer returns to siccit. It remains stationary at the degree where the hygometrical affinities of the hair and the fire acquire an equilibrium. This degree varies, we know, according to the temperature, or what I call the *tension* of fire.

They take  
place only  
in vacuo.

§ 114. But in order that the phenomena may succeed with this promp-

\* Which is also one of the great advantages of this instrument.

titude, I would say with this *elegance*, the globe must be void of air, that the fire may be at liberty to move with the water it carries along, without any obstacle. For if it be fettered by the presence of air, if it be forced to *sift* itself, as I may say, through this gross fluid, which it penetrates with difficulty, particularly when it is united with water in a state of elastic vapour, then all these phenomena are retarded, and the hair, which has time to follow the fire itself in its hygrometric progress, no longer offers the appearances I have just described.

§ 115. These phenomena are the more striking in proportion as the <sup>They in-</sup> absolute quantity of evaporated water <sup>crease with</sup> <sup>the quanti-</sup> <sup>ty of water</sup> <sup>diffolved by</sup> is greater in the globe. However, I <sup>the fire.</sup> have observed them in a degree of siccity,

siccity, where in a temperature— $1^{\circ}$ , the hygrometer was at  $18^{\circ}$ . On approaching the apparatus towards the fire, the hygrometer began to move towards humidity; and afterwards, when the temperature became uniform, it returned to its true point ( $18^{\circ}$ ). But when the variations of temperature are very slow, and when the quantity of moisture in the vacuum is inconsiderable, the hygrometer requires a sufficient interval of time to recover the exact equilibrium of the ambient medium. It required, for instance, 12 hours in this last case.

The presence of air obstructs and delays the effects I have just now described, yet it does not destroy this effect. § 116. Although the presence of air obstructs and delays the effects I have just now described, yet it does not absolutely prevent them. These phenomena explain a fact which, without doubt, has been before observed

served by others, but of which, I think, no satisfactory reason has yet been given.

Cellars and subterraneous vaults, at a certain depth, are commonly dry in winter, and very wet in summer. This observation, which has but a very few local exceptions, ought not, after what has been said, to surprize us. For, in summer, the atmosphere is hotter than the interior of the earth, and fire, which always tends to an equilibrium, moves, in this season, from above downward in the upper strata of the earth, with the water it bears. It deposits this water in these strata successively, in proportion as it penetrates them, and finds them colder. Thus they become loaded with humidity to a certain depth, and retain it until by a change in the temperature of

of the atmosphere, which the cold season brings with it, the fire returns from the earth to the air, and gradually carries with it the water it had deposited during the summer. Hence the *maximum* of dryness in cellars will be observed in the spring, and their extreme moisture in autumn. The nature of the soil, and the depth of the subterraneous caves, may to a certain degree modify these consequences, but they will always be found conformable in fact.

*Even mercury evaporates with facility in the vacuum* § 117. Another fact which, without doubt, has been observed by many people, shews with what energy fire effects evaporation, when not restrained by the presence of the atmosphere. In the upper part of barometers, well purified from air, and exposed to considerable changes of temperature, as for

for example, at a window upon which the sun shines, we see the mercury raised and deposited in little drops on the empty part of the tube. These drops gradually increase, and at length, by their weight, fall back into the mercury from whence they issued. This is a real distillation which takes place in the ordinary temperature of the atmosphere. The fire, although of a density very inconsiderable in this instance, raises the mercury, which is nearly 14 times heavier than water, carries it to the height of at least 2 inches, and deposits it, when by a tendency to equilibrium the fire is urged to traverse the glass, that is to say, on the coldest side, which, in summer, is commonly that opposite to the window.

Conse-  
quence of  
these facts.

§ 118. These facts, as also that of distillation, always so easy and speedy in *vacuo*, and sometimes impossible in air, according to the apparatus we employ, as well as other analogous observations, have impressed me with such ideas of the power of fire in every thing which relates to evaporation, that I am tempted to look upon it as the sole agent in this class of phenomena, and to renounce the idea, that air acts in the manner of chemical *dissolvents*. The very specious arguments of my learned colleague in his hygrometry, in favour of that opinion, had long seduced me; but the charming simplicity which the theory of evaporation would acquire, if we could divest it of the agency of air; the possibility I perceive of reducing the whole to the action of fire; the proba-

probability which increases with the simplicity of every natural hypothesis, attract me, I confess, still more forcibly. At all events, it could only be as a physical agent, or as acting by affinity of *cohesion*, that I could be persuaded to admit it, in any thing which concerns evaporation, agreeably to the distinctions I have insisted on in the first chapter of this Essay.

§ 119. The facts already enumerated in this chapter are not the only phenomena, which the introduction of vapours, whether aqueous or ethereal, into my apparatus has afforded me opportunities of observing. I have very slightly touched upon the *manometric* or elastic effects of these vapours. They require some explanation, and merit, indeed, a course of experiments directed solely towards their examination,

Other ob-  
servations  
relative to  
the elastic-  
ity of va-  
pours.

amination, for which my apparatus would be very convenient.

It may, perhaps, be remembered, that this apparatus contains a gage or barometer in form of a syphon, described § 75, and designed to mark the degree of vacuum obtained in the globe, as well as the elastic force of the vapours which might be introduced. The lateral pipe r, fig. 2, described § 79, furnished me with the means of introducing into the globe known quantities of any substance susceptible of evaporation. I placed this substance, water for example, in a very small glass tube, which I substituted for the large syphon EE. The tube was closed at its outer extremity, and luted at the other to the cock r, the key whereof it touched interiorly. By turning this key a communication was

was formed between the inside of the globe and the little tube, and occasioned the evaporation of the liquid which it contained, and which had been previously weighed with the tube to the precision of  $\frac{1}{75}$  of a grain.

If the liquid, and the inside of the little tube that contained it, had not been freed from air by ebullition, immediately before it was exposed to the vacuum, it would have rushed into the globe at the instant the cock was turned, by the impulse of the elasticity of the air dispersed through it, which was now no longer compressed by the weight of the atmosphere.

§ 120. We have already seen, § 94, Experiment on the elasticity of a given quantity of water in a state of vapour. that the aqueous vapour introduced into the globe in sufficient quantity to cause the hygrometer to rise from  $17^{\circ}$

to 93°. that is, 76 degrees, in a mean temperature of about 7°. had raised the gage from 1,4 lines to 4,5, that is, 3 lines  $\frac{1}{10}$ , which shews that the vapour produced an elastic fluid capable of sustaining, at this temperature, a column of 3,1 lines of mercury.

But at that time, I knew not the absolute quantity of water, which, in a given temperature, would produce a certain manometric effect, or sustain a given column of mercury; I endeavoured, therefore, to determine it by the following experiment.

The thermometer being at + 3,1 and the hygrometer within the globe at 17,3. which announced a considerable degree of dryness, I adapted to the cock the little glass tube, containing exactly a grain and  $\frac{1}{10}$ , or  $\frac{17}{10}$  of a grain of water, which being boiled in

in the tube itself, and the gage being then at 1 line  $\frac{4}{100}$ , I observed as follows :

h.	min.	sec.	Hygr.	Gage.	Ther.
11.	44.	0.	17,3	1,45.	3,1. Opened the lateral cock,
		40.	18,0	..	.. having first adapted the
	46.	15.	19,0	..	.. tube to it still warm.
12.	13.		29.	2,25.	3,3.
	25.		39.	2,25.	3,5. Turned the cock, and
	42.		48.	2,55.	3,0. removed the tube, which
1.	12.		56.	2,56.	3,6. was very dry and quite
1.	40.		58,3	..	3,9. empty.
2.	28.		60,0	..	3,5.
5.	30.		60,7	2,63.	4,2.

§ 121. On examining these <sup>reflections.</sup>   
 many results, we see, 1st, that at the mean temperature of about  $3^{\circ}$ , 2. the tube containing the water being warm, the  $\frac{17}{100}$  of a grain were evaporated in about 40 minutes in the vacuum.

2dly, That when this water was equally diffused in the globe, which did not happen in less than about 6 hours, the hygrometer rose towards

humidity from 17,3 to 60,2 viz. about 43 degrees.

3dly, And finally, that the elastic fluid produced by the union of fire with this water, at the temperature of 4°,2. raised the gage from 1,45 to 2,68 lines, that is to say, sustained in this case, in a permanent manner, 1,23 lines of mercury.

This column of mercury is  $\frac{1}{27}$  of 27 inches, the mean height of the barometer at Geneva, and supposing that the elastic vapour, which sustains it, was common air, and this air to occupy, nearly to the last degrees of dilatation, certain spaces which are in an inverse ratio to the compressing weight, a quantity of air charged with an atmosphere equivalent to 27 inches of mercury, and of a volume equal to  $\frac{1}{27}$  of that of the globe, introduced into

into this globe, would have produced the same effect as the elastic vapour. Now, this volume being 4,55 cubic inches, is, therefore, the same which the vapour, that has filled the globe, would have occupied, supposing it was charged with the weight of the atmosphere, and its volume, *ceteris paribus*, to diminish in proportion to the compressing weight.

Thus  $\frac{1}{7}$  of a grain of water in a liquid state occupies a space of only 0,0032 of a cubic inch. This water, therefore, acquires, by passing into an elastic state in the temperature of this experiment, a volume 1422 times more considerable, supposing it charged with the weight of the atmosphere; and as, in fact, it is not exposed to this weight, and only sustains the  $\frac{1}{73}$  part of it, viz. 1,23 lines, and

Q3 occupies

occupies the whole capacity of the globe, that is, 1197 cubic inches, its liquid volume is to its elastic volume, in these circumstances, as unity to 374<sup>0</sup>63.

This calculation, which is founded on the supposition of a permanent and perfectly elastic aeriform fluid, is not rigorously, nor, indeed, nearly, applicable to the watery elastic vapour, because the pressure it experiences by the re-action resulting from its own elasticity, is even an obstacle to its production, and we soon find the limits where, with a given quantity of water, and in a certain temperature, the formation of the elastic vapour will cease in a close vessel void of air. I believe this term is the same with that of hygrometric saturation, and if so, if the fire and the hair refuse at the

the same time, the one to evaporate more water, and the other to admit into its texture any farther quantity of vapour, evaporation comes within the class of hygrometric phenomena ; it is the result of a pure hygrometric affinity between fire and water, perfectly similar to that of hair, and is likewise a reason for separating this sort of union from the chemical union, with which, as I have already said, many celebrated philosophers seem disposed to confound it.

§ 122. I was desirous of trying Inquiry into the progressive course of evaporation. what progression the formation of watery elastic fluids would follow, by exposing to evaporation in the globe, successively, equal quantities of water in an uniform temperature. For this purpose, I added, by the means already described,  $\frac{1}{5}$  of a grain of wa-

ter to the  $\frac{17}{16}$  previously introduced, which made in the whole  $\frac{14}{16}$ , and after a space of 8 hours, the instruments being all stationary, I observed the hygrometer 72,5 the gage 3,30 and the thermometer 4,2. I then added  $\frac{7}{16}$  of a grain more, and at the expiration of an equal interval, observed the hygrometer 80,4 the gage 4,0 and the thermometer 4,3. At this period an accident gave admission to the air; and I was unable to obtain successively, as I proposed, the term of saturation in *vacuo* at this temperature, from which I was still distant about 20 degrees, but these observations were sufficient to shew me,

1st, That the progression of the hygrometer, alike, in this respect, to that which Mr. De Saussure had observed in the air, decreased relatively to

to the real quantities of moisture introduced into the vacuum. The observations are not sufficiently numerous to establish the law of this progression; and, besides, it was not the object I had in view. It is, however, evident, that  $\frac{1}{2}$  of a grain having raised the hygrometer 43 degrees,  $\frac{1}{6}$  should have raised it about 18 degrees, instead of 11,8; and that the first  $\frac{1}{6}$  having raised it 11,8, the  $\frac{1}{6}$  added in the third experiment, only raised it 7,9 degrees.

§ 123. The gage seems, on the contrary, to shew an increasing progression of elasticity, relatively to the real quantities of water converted into elastic fluid. This circumstance surprised me, and I suspected that a small quantity of air might have entered the globe; but the gage remaining

Remarkable circumstance.

ing 24 hours stationary, destroyed this suspicion, and I concluded that  $\frac{4}{7}$  of a grain of water reduced to elastic fluid having raised the gage from 1,45 to 2,68 lines, that is, 1,23,  $\frac{2}{7}$  ought to have raised it to 3,19 only, instead of 3,30, and, consequently, that  $\frac{3}{7}$  should have raised it to 3,69, instead of to 4,0. If, therefore, we examine with attention these three observations, to the exactitude of which I know no objection, we shall see that they agree in shewing an increasing progression in the elasticity of the watery vapour, formed in vacuo by successive additions of equal quantities of water at the same temperature. And we shall presently shew, that the small variation of the thermometer in these three experiments cannot account for this fact.

§ 124. I made, at the same time, some experiments on the influence of temperature on the elasticity of this vapour, and I saw, that although the variations of temperature acted powerfully upon the hygrometer, they had little effect upon the gage; but that they increased, however, with the quantities of water evaporated. Thus in the first experiment with  $\frac{1}{6}$  of a grain of water, an increase of  $7^{\circ},6$  of temperature made the hygrometer descend from  $60^{\circ},2$ , to  $47,5$ , that is,  $12^{\circ},7$ , and the gage remained almost stationary. In the last experiment, in which there was  $\frac{3}{6}$  of a grain of water evaporated in the globe, an increased temperature of  $11^{\circ},4$  viz. from zero to  $11^{\circ},4$  made the hygrometer descend from  $92^{\circ},3$ . to  $60^{\circ}$ , that is, it moved towards siccitry  $32^{\circ},3$ ., and the gage rose

rose from 3,80, to 4,15 lines, that is, 0,35.

It would, indeed, form an object of very interesting experiments, to observe the correspondent progressions of the thermometer and the gage or manometer in *vacuo*, answering to equal and successive increases of temperature within the limits of atmospheric heat, and at the same time to study, by the joint means of the manometer and hygrometer, the two simultaneous effects of fire, viz. the augmentation of its hygometrical affinity with water in proportion as its density increases, and that of the elasticity which it gives to the water it evaporates; and lastly, to determine according to different temperatures, what may be the absolute quantity which, in a given vacuum, would be the

the *ne plus ultra*, or the term where the elasticity of the fluid already evaporated would hinder, by its own pressure, any further evaporation. But I have been too prolix on this subject, if I aimed only at pointing out the road to other observers, and on the other hand, I have not tried a sufficient number of experiments to make it clear. But these matters will become the subject of my approaching labours,

§ 125. Some particulars concerning <sup>Particulars</sup> the phenomena which accompanied <sup>concerning</sup> the introduction of the ethereal <sup>the ethereal</sup> vapour. <sup>vapour.</sup>

It was impossible to weigh the ether, as I had weighed the water, on account of the great volatility of this fluid: I contented myself, therefore, with counting

counting the drops I introduced into the glass tube, luted to the lateral cock ; I put in 17 at a time, which were darted into the globe with a certain hissing noise at the instant the cock was opened.

The instruments hygr. gage. ther. being previously examined, were 21,8. 1,50. 10,5

At the expiration of six minutes from the introduction of the first 17 drops

27,0. 5,0. 10,8..

Three minutes afterwards, I introduced a second dose of 17 drops, and left the cock open for three minutes : the same

hissing

hissing sound was heard at the opening, and two minutes after the cock was turned I observed

39,0. 8,5. 11,0.

Seven minutes after, I introduced a third dose, which was accompanied by the same phenomena; the hygrometer was then at 46,5; at the end of 5 minutes

I observed 69,0. 11,5. 11,0.

And in half an hour after

84,0. 11,9. 11,2.

§ 126. In the first place, we observe by the motion of the hygrometer, that the ether, although I had treated

Reflections.

treated it with fixed alkali, and rectified it carefully in a water bath at  $42^{\circ}$ ., still it contained water, from which it is impossible to purify it by the usual means. We see that 51 drops made the hygrometer rise  $62^{\circ}, 2$ , and if we knew exactly the weight of the ether introduced, and that of pure water which produces the same hygrometric effect in vacuo at a given temperature, we should then have a physical method of knowing the quantity of water which forms an integral part of rectified ether. Finally, the gage shews us, that 17 drops of ether introduced at each time caused the mercury to rise, when they were entirely evaporated, about 3 lines  $\frac{1}{2}$  at the temperature of  $11^{\circ}$ . The elasticity seems to follow a decreasing progression, relatively to the quantities of ether introduced;

troduced; but not having been able to weigh the quantities, the experiment is not sufficiently exact to allow any absolute conclusions like those we have endeavoured to establish with regard to the aqueous vapour.

§. 127. However, I endeavoured to determine the influence of temperature upon the elasticity of ethereal vapour. With this design, the instruments being, hygr. 84, gage 11,9. therm. 11,2, I transported the apparatus into a colder chamber, where it remained 40 minutes, and at the expiration of that time I observed the hygr. 89, gage 10,9 therm. 6,4. The refrigeration of  $4^{\circ}, 8$  produced a descent of the mercury of the gage of 1,9 line. Very little dew was seen upon the sides of the globe, but it appeared to be ethereal and not aqueous, for,

The influence of heat on the elasticity of ethereal vapour.

the hygrometer was only at 89,0 that is, 11 degrees from the point of saturation. I then exposed the apparatus to a higher temperature than the first, and observed hygr. 64,0 gage 13.1 therm. 21,0.

This last experiment presents a very singular result. An elevation of temperature of 10°. viz. from 11°. to 21°. produces an elevation in the gage of only 1,2 line; whereas, in the preceding experiment, made in a temperature between the 6th and 11th degree, a difference of only 4°,8. less by half than the variation in the last experiment, produced a difference of 1,9. line. Perhaps this irregularity may be owing to the insufficiency of the temperature, in the last case, for the evaporation of all the ether; and therefore a part of it may have repassed

from

from its elastic state to a state of liquidity. It is possible, that this transition is made suddenly, or by a spring which deranges the regular progression of the elasticity. This is likewise a point to be pursued; but I repeat to those, who may have such intention, that the ethereal elastic fluid attacks the varnish of the instruments exposed to it, and in a certain time even spoils the hair of the hygrometer.

column of air intercepted between the two stations, in which the barometer is observed ; for it is evident, that by observing, as we usually do, the temperature of a thermometer suspended at 5 feet from the earth on a plain, and on a mountain, we obtain the mean heat of the stratum of air which lies on the earth to the distance of five feet from its surface from one station to the other, rather than that of the column which is raised vertically upon the inferior station.

Difficulty of  
these obser-  
vations.

§ 130. If we could investigate by experiment the law by which heat diminishes in proportion as we rise vertically in the air, according to the various seasons of the year, or according as the temperature of the atmosphere is modified by the presence of the sun, of clouds, or of wind, we

we should go immediately to the point, and obtain satisfactory approximations.

But these observations are neither so simple nor so easily made as may at first be imagined. Let them be made on the summit of a mountain, or on the top of a steeple, they will still be somewhat affected by the heat reflected from the steeple or from the mountain ; and will not exactly shew the temperature of the horizontal stratum of air, by which these objects are enveloped. The only means remaining are, a balloon of inflammable air, large enough to carry an observer, and this method is not easily practicable ; or lastly, a high pole at whose extremity a thermometer may be suspended, which may be rapidly lowered

for observation. This is the method I have employed.

*Description of the apparatus made use of* § 131. In some former experiments I made use of a pole 50 feet high, and afterwards of one 75 feet high. My first trials were made in the months of August and September, 1778, and discovering some phenomena I did not expect, I repeated them the year following with better instruments, and a more convenient apparatus. And it is of these latter experiments I am now about to give an account.

The pole was fixed in a large garden, and supported in its vertical situation by cords or shrouds descending obliquely to the ground. From the top of the pole, an arm of about 18 inches long was extended horizontally, from whose extremity hung a pulley designed to raise and lower one or more

more thermometers, which was done with the utmost celerity. The upper part of the pole was painted a dead black, in order to avoid all kind of reflection ; and that the shadow of the pole should never fall upon the thermometer, I placed the arm in the plane of the meridian.

Besides these thermometers destined to be raised or lowered, I employed others, suspended at different distances from the earth, from between 5 and 6 feet, to 4 lines. These were suspended by a very thin thread of silk extended vertically ; one of them was hung two inches from the pole itself and 5 feet from the earth. I moved it laterally as the sun advanced, that it might be always diametrically opposite the pole, and by that means constantly in the shade. And finally, I had

I had one on the ground, with its bulb just covered by the earth, intended to shew the temperature of the surface of the soil, whilst the corresponding observations were made in the air.

The instruments were all of mercury, made with great care, and agreeing perfectly with each other. The bulbs and inferior parts were completely insulated and detached from their scales, which were of tin, silver, or ivory. The bulbs were of a moderate size, so as to require 6 or 7 minutes to receive the temperature of the quiet air in which they were suspended. The most tardy was disposed at the top of the pole, that its variations might be as inconsiderable as possible during the 5 or 6 seconds it required to descend.

§ 132. My design, in general, was to observe, by means of this apparatus, what passed in the stratum of air which repose upon the earth to the height of 75 feet; to see what progression the augmentation and diminution of the heat produced by the sun's presence, during a calm and serene day, would follow; what was the hottest and coldest moment of the day; what the mean heat of the 24 hours; and finally, to examine the effect of clouds, fogs, winds, &c. upon these results. But above all, I endeavoured to discover if there existed any constant proportion between the temperature of the air at 75 feet, and at 5 feet from the earth; and, supposing this proportion variable, what might be the nature and periods of these variations. Then to apply these

1

Particular  
object of  
these expe-  
riments.

these results to the nearest approximation of the true temperature of a vertical column of air drawn from the ordinary and only practicable observations, made at 5 feet from the earth.

A more considerable elevation would, perhaps, have furnished the means of establishing, by numerous observations at intermediate heights, some law of the diminution of heat, relatively to the augmentation of vertical elevation; but a column of air of 75 feet was insufficient for such inquiries.

**General results.** § 133. It was at the top of the pole that the augmentation and diminution of heat during the day followed the most uniform progression; and it was there, likewise, as we shall presently see, that the extremes of heat and cold were nearest each other. The thermo-

thermometer, in the shade, at 5 feet from the ground, agreed best with the thermometer, exposed to the sun, 70 feet higher, and they were not only similar, in their progression, but in their absolute temperature also, from 9. o'clock in the morning to 3 in the afternoon, although one was in the sun, and the other in the shade.

§. 134. I usually began to observe these instruments at break of day, <sup>Greatest coolness bout sun rise.</sup> and they all agreed in indicating an increasing coolness as sun rise approached. The coldest moment was during the rising of the sun; and from that time the thermometers began to mount by different progressions, until about 3. o'clock in the afternoon, which was commonly the hottest part of the day. The thermometer, whose bulb was covered by the soil, indicated

cated at that time a very considerable degree of heat. I have seen it at 45°. of the scale of 80 parts, in a hot day in the month of August.

The winds greatly altered the uniformity of the progression of the thermometers, which, on the days when the air was agitated, moved always by oscillations : a cloud which hid the sun, also occasioned a sudden descent ; but their motions were never more regular than on those calm and uniformly cloudy days, which we frequently see in this country in Autumn.

**Particular phenomenon.** § 135. All these modifications were naturally foreseen. The particular results of these observations, as they relate to barometrical measurements, will make part of another Essay directed principally to inquiries of this nature.

nature. But the peculiar and unexpected phenomenon of which I am now going to speak, has naturally a place here.

In order to expose it clearly, I shall follow the course of the two thermometers at 5 feet and at 75 feet from the earth during 24 hours of calm serene weather.

In the morning, about two hours or two hours and a half after sun rise, these two thermometers agreed, and, except some little oscillations, the effect of accidental circumstances, they indicated the same temperature.

As the sun advanced the thermometer at 5 feet from the ground acquired a higher temperature, and at the hottest time of the day, it was about two degrees of the scale of 80

parts

parts higher than the thermometer at 75 feet.

The *maximum*, of difference, once pasted, the two thermometers approached, and, before the setting of the sun, again acquired the same temperature; then varied in the opposite sense, and their difference augmented rapidly after sun set. Towards the end of the twilight, the inferior thermometer was two degrees lower than the other, and sometimes more.

This difference continued the same during the night; at least I have reason to presume so; because, having quitted them at 11 o'Clock in the evening, and observed them again at day-break, I constantly found the thermometer at 5 feet, lower from one to two degrees than that at 75 feet. They kept the same proportion during

during the whole twilight of the morning, and it was not till sometime after sun rise that they began to approach, then to acquire the same temperature, and about two hours afterward to cross each other again.

Such was the constant course of these two thermometers, as often as the weather was calm and serene ; and it happened just the same in the different seasons of the year, and notwithstanding winds and clouds, although less sensibly in this last case ; and it was only on the days completely and uniformly cloudy, and when the wind was violent, or when there was a thick fog, that the two thermometers, 70 feet distant from each other, nearly agreed during the whole course of the day.

It is far-  
prizing.

§ 136. It was with extreme surprise, that I beheld, from the very first day of my experiments, this extraordinary phenomenon. I thought, and doubtless I was not singular in this opinion, that the coolness which we experience in the night came from above; nor could I believe my eyes when I saw the thermometer at 75 feet, at that time, nearly two degrees higher than the other at 5 feet. It was from the ground, then, I concluded that this coolness proceeded. And in effect, I found the thermometer at 4 lines from the ground, still lower, in general, than that at 5 feet; but the thermometer, whose bulb was covered by the earth, was, on the contrary, much higher than any of the others; and the earth, retaining a part of the heat which it had acquired

quired during the day, formed, as it were, a kind of stove, immediately whereon rested a stratum of cool air, and above that we found the air warmer.

§ 137. It may be imagined, that this was a local phenomenon, and owing to some particular exhalations; but the soil was not at all moist; the same experiments repeated in a larger plain, to which I transported the apparatus, presented the like results; and I have even obtained the same upon an insulated summit of the mountain called the Mole, which is more than 4200 feet above the level of the sea.

§ 138. I think I perceive the cause of this phenomenon to be as follows : Suspicion of its cause.

When fire is not retained by the ties of chemical affinity, it tends con-

stantly to an equilibrium, as we have already often proved in the course of this Essay. The soil, therefore, being hotter than the air, the fire will rise from the interior of the earth upwards, and, being arrived at the surface, will successively convert into vapour the infinitely thin strata of water which moisten the soil in the point of contact between the air and the earth, and evaporation will take place.

If we suppose the ground colder than the air, then the fire will descend into the earth; but evaporation will always take place near its surface; and in this particular union of fire with water, it is known that a part of the sensible fire disappears, and becomes fire of evaporation, that is to say, imperceptible fire, and that cold is

is produced, unless in circumstances in which the external calorific causes exactly counterbalance the fire that disappears.

During the two twilights and the night, this balance cannot take place. The ambient liberated fire endeavours to re-establish the equilibrium, but the fire that arrives for this purpose from the earth undergoes at its surface the metamorphosis produced by the act of evaporation, and that which exists in the air above is partly united with the water it has evaporated, and not being carried downward by any great rupture of equilibrium, but having, as we have perceived in another place, a tendency, independent of the attraction of the superior strata, to rise rather than to descend, it moves through the air in a quantity insufficient

cient to re-establish in the inferior stratum the equilibrium of temperature deranged by evaporation. It will, therefore, remain colder than the ground, and than the superior stratum, till some calorific cause shall supply the fire, which disappeared at the surface of the earth.

Effects of  
the sun's  
rays.

§ 139. But as soon as the rays of the sun fall upon the inferior stratum of air, and upon the surface of the earth, their calorific effect is felt. In the first moments, they produce cold rather than heat, because the fire they supply is not, perhaps, sufficient to compensate the quantity absorbed by the increased evaporation.

Their presence, however, and more direct impulse upon the air and the ground soon gain the advantage, and, notwithstanding the increased evaporation,

ration, the liberated fire is so much augmented, that the relative heat of the inferior stratum becomes greater; and this stratum and the superior strata also to the height of 75 feet, which are the limits of these experiments, shortly acquire the same temperature that took place about two hours or two hours and a half after sun rise. The heat produced by the action of the solar rays upon the earth, became afterwards so great, that the inferior stratum of air, in which, however, the evaporation constantly continued, was heated more than the superior, and we have seen the difference amount to 2 degrees in the hottest time of the day.

After this period, the diminution of the intensity of the solar rays brought back, by degrees, the equality be-

tween the refrigerant effect of evaporation, and the calorific effect which they produced ; and at this moment of equilibrium, the superior and inferior thermometers again exhibited the same degree of temperature ; a little afterward the rays, still more oblique and less numerous, were insufficient to counterbalance the cold of evaporation, and the inferior thermometer descended lower than the other ; this difference continued till the next day's sun restored the equilibrium, and produced the alternate action which I have described and endeavoured to explain.

*Reflections.* § 140. But should this explication be insufficient, the fact is not less certain ; and if we add to the conclusions that may be drawn from it, the knowledge we have of the three states or modifi-

modifications of fire in solid water, liquid water, and water in vapour, the phenomena of dew and of hoar frost will not be very difficult to explain in a satisfactory manner. But it would carry me too far to develop these ideas, and they will be found in another place.

§ 141. I cannot, however, pass in silence two facts which have an immediate relation with these experiments.

Mr. De Saussure, amongst other lights thrown on the theory of evaporation in his excellent work on hygrometry, has most happily arranged the vapours suspended in the air under three states, which he has distinguished by the denominations of *pure elastic vapour*, *vesicular vapour*, and *concrete vapour*. The first is invisible in

in the air; the second forms little empty vesicles which occasion fogs and clouds; the third forms round drops or common rain; and these three modifications succeed one another according to circumstances.

But are the relative quantities of fire employed to produce them equal? And as water contains less fire when solid than when liquid, and still less when liquid than when it is in an aeriform state, does not the vesicular vapour require less fire than the pure elastic vapour?

Mr. De Saussure presumes with reason, that this vesicular vapour contains more fire than the concrete vapour, and in proof he instances the rains of winter, which sensibly heat the inferior part of the atmosphere; because in the conversion of the vesicular

cular vapour into rain, the fire, superabundant to this new form, becomes sensible fire, and by its adherence to the molecules of water, is carried towards the earth.

But does this phenomenon take place in the transition of water from its state of invisible elastic vapour to that of vesicular vapour? Experiment alone can resolve this question; and it required all the chance of a particular meteorological event to enable me to make this experiment in the atmosphere.

§ 142. This event presented itself in the course of my experiments on the 18th of October, 1779, in the twilight of the morning. I had a fog at the top of the pole during several minutes before it formed near the earth, and at the moment the fog appeared

Remarkable circumstance.

appeared near the top of the pole, the thermometer rose  $\frac{1}{8}$  of a degree. We shall have a more perfect idea of this phenomenon by casting our eyes upon the following table, copied from the journal of observations.

1879 Oct. 18 in the morning	Therm. covered by the soil.	Therm. at 5 feet from the ground.	Therm. at 75 feet from the ground.	Remarkable Events
h. min.				
6. 20.	4,8.	4,2.	6,0.	Serene, calm, little fog.
30.	4,8.	4,5.	6,1.	Idem.
40.	5,0.	4,3.	5,3.	Fog a little increased.
50.	5,0.	4,2.	6,0.	Fog at the top of the pole, and none below.
7.	5,1.	4,4.	5,9.	Fog universal, but not thick
2.				Sun rises, but very pale.
15.	5,4.	4,9.	5,8.	Fog disappears. Sun shines weakly.
30.	5,5.	5,0.	6,1.	Idem.

It appears pretty clearly, by the course of observations of the thermometer at 75 feet, that the appearance of the fog at the top of the pole caused a momentary heat.

§ 143. This heat, which is disengaged, and which remains sometimes mingled in the vesicular vapour, explains, in some measure, why cold is never so rigorous in cloudy weather as when the sky is serene; because in clear weather, the rupture of equilibrium, between the temperature of the superior and inferior strata of the atmosphere, may be considered as causing a continual current of fire from below upwards, which deprives both the surface of the earth and the neighbouring strata of the liberated part of this fluid, in a more rapid manner than the soil itself can furnish it.

it. For fire, I must repeat it again and again, cannot be restrained or *cohibited* but by itself. If, therefore, a cloud appears at a certain height, the fire it carries with it stops the current of which I speak, makes it recoil, on the contrary, below, and causes that, which continues to escape from the ground, to accumulate in the inferior strata of the air, and soften its temperature.

Another fact which these experiments explain.

§ 144. These experiments likewise afford the solution of a difficulty which is of importance from its immediate connection with the measurement of heights by the barometer.

Those who are acquainted with this subject, and who have read the excellent work in which Mr. De Luc has developed this method and its applications, will recollect his having announced

announced that the observations made about sunrise, and in the hottest part of the day seldom agreed with his rules. The first constantly making the heights too little, and the latter, in general, too great.

Now it follows from the facts I have related, that Mr. De Luc and all those, who observe the thermometer at 5 feet from the ground at sunrise, suppose the air colder than it really is, since even at 70 feet higher, it is sometimes warmer by two degrees; therefore, the subtractive correction directed by Mr. De Luc is made too considerable, and the altitudes obtained are consequently too little. The contrary happens in observations made at the hottest time of the day; the thermometer at 5 feet from the earth shews an accidental heat, which does

not

not exist a few feet higher; and as they conceive the whole column between the two stations to be hotter than it really is, the subtractive correction is made too little, or the additory too great, and consequently the heights obtained are too considerable.

A difference of about  $2^{\circ}$ . of the common thermometer, or of near  $5^{\circ}$ . of the scale adopted by Mr. De Luc, which frequently takes place between the thermometers at 5 feet and at 75 in opposite directions, is sufficient to affect the results considerably. It is remarkable that the time he mentions as most favourable for the exactitude of barometrical observations, viz. the fifth part of the sun's stay above the horizon, is also nearly the time when the course of the two thermometers cross, and when they agree for some moments,

moments, and consequently the most eligible to judge of the temperature of the atmosphere at a certain height, by an observation made near the earth, and by this means to approach the true mean temperature required. But, as I have already said, it is necessary to make the observation on the thermometer in the shade.

§ 145. There is also another object of enquiry relating to temperature, on which these experiments may throw some light: I mean the regular diurnal progression of atmospheric heat in different seasons of the year, and in particular the true mean heat of the 24 hours.

The manner of estimating this last is not generally agreed upon. Some suppose it the arithmetical mean of the highest and lowest degrees of the

thermometer observed in the 24 hours, without regard to the duration of the intermediate temperatures. Others, the mean heat of three periodical observations made in the morning, at noon, and in the evening.

The true mean heat would result more exactly from the sum of the degrees of an infinite number of observations made in the 24 hours, divided by the number of the observations themselves. And the more this principle is adhered to in the process which may be pursued for its determination, the nearer will the result approximate the truth.

*Procedure.* § 146. In the course of my experiments on the temperature of the atmosphere; I have more than once observed the thermometer every half hour, from the dawn of day till 10 o'clock

o'clock in the evening: sometimes I have observed every quarter of an hour during the same interval; and by adding to the observations actually made, the changes of temperature which may be supposed to happen during the night, from ten o'clock in the evening till the dawn of day, and which would probably follow a regular decrease in an arithmetical progression, we might thus compute the mean temperature of the 24 hours from 48 observations; or from 96 when made every quarter of an hour.

Agreeably to this method I have chosen from my register of observations, those which were made on very serene and uniform days, in the hottest season, and also those made about the vernal equinox, in order to ascertain the mean heat of the 24 hours.

at these two periods. The observations made the 16th August, 1779, will represent pretty well the temperature of an ordinary summer's day in our climate; and those made the 19th March, 1781, a day in the beginning of spring.

Mean tem-  
perature of  
a summer's  
day.

§ 147. On the first of these two days, I had 48 observations, including the variations I supposed to have taken place from 10 o'clock in the evening till 40 minutes past 4 in the morning. I had left the thermometer at  $14^{\circ}$  at 10 o'clock in the evening, and finding it at  $10^{\circ}$ , at 30 minutes past 4 in the morning, I divided the  $3^{\circ}, 9$  between the 6 hours and  $\frac{1}{2}$  which were elapsed, and thus formed so many supposed observations, which probably were not far from the truth, and whose possible variation could have but

but a very trifling influence upon the mean of the 24 hours.

By this procedure, I found the mean heat of the 24 hours deduced from the 48 observations taken in the shade at 5 feet from the earth, to be  $16^{\circ}, 1.$

In the course of my observations, I found that the thermometer indicated this temperature about 8 o'clock in the morning, and about  $7 \frac{3}{4}$  in the evening. If, therefore, one should be desirous of obtaining an idea of the mean heat of a clear summer's day by a single observation, this observation should be made at one of the hours just mentioned.

The mean of the extreme temperatures observed that day, viz. at sunrise, and at 3 o'clock in the afternoon,

T 3                    gave

gave  $16^{\circ},05$  nearly approaching that of the 24 hours found above.

The mean of 3 periodical observations made the same day, viz. at sunrise, at sunset, and during the hottest moment of the day, gave  $16^{\circ},5$ .

And the mean of 3 observations, made at sunrise, at noon, and at sunset, gave  $16^{\circ},1$ . which agreed perfectly with the mean of the 24 hours.

The difference between the hottest and coldest time of the day was  $12^{\circ},8$ .

§ 148. By combining in the same manner the observations of the 19th March, 1781, made every quarter of an hour, viz. to the number of 96 during the 24 hours, we find the following results :

The mean heat at 5 feet from the earth in the shade was  $5^{\circ},8$ ; and we found this degree of temperature at

8 o'clock

And of a  
day in  
spring.

8 o'clock in the morning, and at 10 in the evening. It is remarkable, that in seasons so different the mean heat should be equally represented by the observation made at 8 in the morning. But the mean between the extremes of temperature on this day gave  $7^{\circ},9$ . which exceeds the mean of the 24 hours  $2^{\circ},1$ . The mean of three periodical observations made at sunrise, at sunset, and in the hottest time of the day, gave  $9^{\circ},3$  ; and the mean of 3 observations made at sunrise, at noon, and at sunset, gave  $8^{\circ},8$  ; which exceed the other mean still more. The difference between the extremes of temperature this day was  $14^{\circ},8$ .

From these examples it may be presumed that it would be difficult to give any simple formula, applicable

to all seasons, which, from two or three observations made at certain periods of the day, would nearly shew the mean temperature of the 24 hours.

*A graphical method very convenient for these inquiries.* § 149. In my daily observations, I have found nothing more convenient for obtaining a clear idea of the diurnal course of atmospheric heat, than after the example of other philosophers, to represent it graphically by means of curved lines, upon the axis of which I marked abscisses proportioned to the times elapsed between the observations, and the correspondent ordinates thereof represented the elevation of the thermometer for every observation. The form of the curve which passed through the extremities of these ordinates, and which became a straight line oblique to the axis in the

the supposed observations between 10 o'clock in the evening and day-break in the morning, described to the eye, in the most compleat and satisfactory manner, and in an instant, what could not have been discovered without much time and infinite attention in columns of cyphers representing the observations themselves.

§ 150. This curve presented an irregularity in the part corresponding to the observations made towards 9 o'clock in the morning in summer; it there formed two or three zig-zags, occasioned by a light breeze which generally rose about that hour in the serene days, and cooled the atmosphere by intervals. The curve afterwards regained its regularity, became parallel to the axis between 2 and 3 o'clock P. M. and from that time till

fun-

Curve  
which re-  
presented  
the obser-  
vations.

sunset approached it more briskly than it had diverged from it in the morning. The curves of different seasons have each their characteristic. Those of the spring are most convex, because the extreme differences of temperature at this season of the year are greatest.

But I am wandering from my subject. It shall, however, be resumed in the following chapter, which will conclude this essay.

## C H A P. IX.

*Experiments on Heat produced by Friction.*

§ 152. IF the discovery of a truth is a source of real pleasure to a philosopher, an opportunity of correcting an error which he had adopted will also be a gratification to a candid mind, and is what I experienced in the course of experiments contained in this chapter.

I was persuaded that the heat produced by friction was owing, in a great measure, to a kind of mechanical decomposition of the air between the surfaces rubbing against each other; and this opinion appeared to be probable

Opinion concerning the cause of heat produced by friction.

bable from the observation that the fragments of steel detached by the stroke of a flint, were not melted in *vacuo* as they were in air. This hypothesis seemed also to be supported by considerations on the quantity of fire chemically contained in this elastic fluid. But we shall presently see, that experience has overturned it.

Apparatus  
for trying  
the truth  
of this opi-  
nion.

§ 153. In order to vary the experiments, I employed the following apparatus.

It is a kind of clock-work movement, 3 inches in diameter, and 2 in height. The moving power is a spring inclosed after the usual manner in a barrel, the wheel of which has 120 teeth, and runs in a pinion of 12 leaves, whose spindle carries a wheel of 98 teeth. This wheel runs in a pinion of 8 leaves, which drives a third

third wheel of 60 teeth, and this turns a third pinion of 22 leaves, which occupies the centre of the machine towards the upper plate. The spindle of this pinion projects above the plate and is terminated by a square, to which the substances designed to make trial of the friction are adapted.

In consequence of such a number of teeth and leaves running in each other, the last pinion makes 334 revolutions for one of the wheel of the barrel.

To this pinion are fitted several little hemispherical cups with the concavity uppermost, and their bottoms being pierced with a square hole corresponding with the square of the spindle, they are placed thereon and turn with it. The cups I made use of were of steel, of brass, and of wood, and of

two different diameters, viz. 7 lines and  $3\frac{1}{2}$ .

Immediately over the centre of the cup fixed for the experiment, I place a mercurial thermometer, whose bulb is only  $2\frac{1}{4}$  lines in diameter. This thermometer may be raised or lowered so as to enter, more or less, the cavity of the cup without touching its sides; by which disposition it is calculated to receive and shew most readily the heat given to the cup by the friction.

The friction operates on the outer edge of the cup near the brim; and in order to vary it either by the degree of pressure, or the nature of the substances rubbing against each other, I employ a horizontal lever, whose point of support is at one of the extremities, the direction of which is parallel to a tangent of the circumference

rence of the cup, and at the other extremity a thread is fixed at right angles, which, passing over a pulley, suspends a weight that may be varied. The lever is furnished with a mouth-piece at its mid-length, resembling that of the cock of a pistol, in which I place the different substances intended to rub against the cup with a pression determinable by the weight. For, as the lever is of the second class, and the distance from the mouth-piece to the point of support is  $13\frac{1}{3}$  lines, whilst the weight acts at its extremity at double that distance, or  $26\frac{2}{3}$  lines, the action of the weight pressing the substance rubbing against the cup is double the weight itself, which being 4 drachms, 18 grains, its effect is consequently equal to 1 ounce, 36 grains; and this is the pression I have constantly

stantly employed in the experiments of which I am about to render an account.

I found the relative velocity of the substances under friction, in the following manner. When I applied the steel cup of 7 lines diameter to the spindle of the pinion, and suspended to the lever the weight above mentioned, putting in the mouth-piece a bit of brass to rub against the steel, the wheel of the barrel in running down makes 5 revolutions in 8 seconds; it may therefore be concluded, that in this case the circumference of the cup is moved at the rate of 32 feet in a second. But when instead of the great steel cup I substitute a brass one of only  $3\frac{1}{2}$  lines diameter, leaving the same pressure and the same rubbing substance, the wheel of the bar-

rel makes one revolution in a seond, which gives the circumference of the cup a velocity of  $25\frac{1}{2}$  feet in a seond. And these are the extremes of the velocities employed in my experiments.

The apparatus is of a size to admit its being readily introduced into the common receivers; and it may be set in motion in vacuo by means of a rod, which traverses the leatherne apparatus commonly employed for this purpose, and the extremity of which touches a spring or trigger.

§ 154. Comparative experiments in the air and in vacuo were the first objects of my inquiries. For this purpose, having adapted a cup of tempered steel, and placed in the mouth-piece a bit of adamantine spar, which mineralogists esteem the hardest substance next to the diamond, I set the

U machine

Experi-  
ments on  
friction in  
the air, and  
in vacuo.

machine in motion in the air. Sparks flew out during all the revolutions, and formed a radiated sheaf of light, whose top was at the point of contact. The thermometer in the centre of the cup, at some distance, however, from its sides, shewed no signs of heat produced by the friction.

I repeated the experiment and placed the bulb of the thermometer without the cup, very near its outer edge, presuming that the fire carried away, perhaps, by the rapidity of the revolutions would form an atmosphere round the edge, and thereby affect the thermometer. But the instrument in this new disposition gave no signs of heat.

I repeated the experiment in a vacuum, where the mercury of the gage stood at 4 lines. The thermometer shewed

shewed no signs of heat in this case, and I saw no sparks. I even thought that no light was produced ; but when I renewed the experiment in complete obscurity, I saw at the place of contact a phosphoric glimmer, like that observed upon rubbing hard stones in the dark.

§ 155. From the result of these experiments I naturally concluded that, in the present disposition of my apparatus, the heat produced by the friction was so weak that the thermometer thus situated could not indicate it in a manner to be depended upon ; for, I could perceive no variation in the experiments made alternately in the air and in *vacuo*, except that in the former I obtained sparks, and in the latter only a feeble phosphoric light.

Change in  
the appara-  
tus.

§ 156. I now substituted the brass cup of  $3\frac{1}{2}$  lines instead of the steel cup of 7 lines, and the bulb of the thermometer almost entirely occupying the cavity of the cup, it was so near its sides, that the smallest heat produced must affect it. I placed in the mouth-piece a bit of brass, and the pressure being always the same, I set the machine in motion in the air several times following, and each time obtained an ascension of  $\frac{3}{10}$  of a degree of the thermometer.

I observed that the thermometer did not begin to mount till the machine had finished its revolutions. Their rapidity, without doubt, gave to the ambient air a tangential force which carried off the fire in proportion as it was disengaged by the friction; but as soon as the motion ceased, the

the thermometer rose during 15 or 20 seconds to a certain *maximum*, which varied, as will be seen, according to circumstances.

§ 157. I repeated the same experiment in a vacuum which supported an inch of mercury, and I obtained a mean ascension of  $1,^{\circ}2$ ; with this difference, that the thermometer rose whilst the machine was in motion, and thereby confirmed the explication I have just given of the cause which rendered it stationary in the air.

In this instance we see fire excited by friction more efficaciously in vacuo than in the air. The specific heat of air, by absorbing a part of the heat produced, tended, without doubt, to increase the difference of these results; but the difference which still remained

was sufficient to induce a suspicion of my mistake.

To ascertain whether the heat I had observed was really owing to the friction of the substance in contact with the outer edge of the cup, I put the cup in motion, without any thing touching its edge, and the thermometer remained perfectly stationary.

The influence of hardness in the rubbing substances.

§ 158. In these experiments we have seen metallic substances of the same kind in contact, and to know whether the hardness of the rubbing substances was one of the causes which contributed to the production of heat, I let the brass cup remain, and placed in the mouth-piece, instead of the bit of brass, a piece of the soft wood of a pencil, which touched the cup by a very narrow edge only. By this disposition I obtained in the air a mean ascen-

ascension of  $\frac{7}{10}$  of a degree, which was greater by  $\frac{4}{10}$  than what had been produced by the friction of brass against brass.

To confirm these results I substituted, instead of the brass cup, a cup of very soft wood of the same diameter, leaving in the mouth-piece the same bit of wood which had been made use of in the preceding experiment. This alteration occasioned an ascension of  $2^{\circ}, 1$  in the air, which was the mean of three experiments that differed only  $\frac{1}{10}$  of a degree from each other. In this case, wood rubbing against wood, produced a heat three times greater than had been obtained by the friction of wood against brass, which perfectly confirmed the first results.

I repeated this experiment in *vacuo*, and had a mean ascension of  $2^{\circ},4$ ; that is to say, a little more heat than in the air, according to the preceding observations.

*Experiment in condensed air.* § 159. Still better to determine the influence of the air, I was desirous of repeating the experiment in the extremes; that is, having already made it in *vacuo*, of trying it in condensed air. The gage of the condensing pump was at 48 inches, and the air within consequently charged with an atmosphere and three fourths. In these circumstances I obtained only  $\frac{1}{10}$  of a degree of heat; whereas I had obtained  $2^{\circ},4$  in a vacuum which supported one inch of mercury, and the position of the thermometer remaining always the same, in a series of experiments made

made with the same cup, no accidental variation could have happened.

§ 160. But chance occasioned the <sup>Singular effect discovered by chance.</sup> discovery of a circumstance which powerfully modified and still more completely overthrew the ideas I had formed of the cause of heat produced by friction.

When I employed the brass cup, I was apprehensive, from the smallness of its concavity, that the bulb of the thermometer, owing to some irregular motion, might be broken by the rapid friction of the metallic body, which it almost touched. And to prevent this accident, I lined the inside of the cup with cotton, which touched very slightly, and by some filaments only, the under part of the bulb. I now saw with much surprise the

the thermometer rise 5 or 6 degrees during the revolutions of the cup.

This phenomenon took place independent of all external friction against the cup, and was therefore manifestly owing to the friction, although very light, of the cotton under the bulb of the thermometer.

I repeated this experiment in many different ways, and observed, that in proportion as I pressed the thermometer upon the cotton, the heat was increased, and to that degree, that the thermometer I used for these observations having only 15 degrees of movement, I was fearful of breaking it by carrying the experiment to the extreme.

There are certainly few bodies more yielding than the filaments of cotton, and yet it was their friction which

which was the most energetic of all the means I employed in producing heat. It is true that the cotton rubbed the bulb of the thermometer itself; whereas all the other frictions had been made only against the cup which surrounded it: But I cannot be persuaded that the difference of the results was solely owing to this circumstance. It appeared, that the heat produced in *vacuo* was, in this case also, greater than that produced in the air; but it is evident, that experiments made in this manner cannot be rigorously comparative, because it is impossible to ascertain a perfect equality of pressure upon the cotton in two consecutive experiments.

§ 161. It is very difficult to reason upon the facts I have just related. Reflections.

They

They offer only some data for our procedure, by way of exclusion. We see that it is not air which causes heat in friction ; we see also that it is not the hardness of the rubbing substances ; but we do not perceive what is the quality of bodies on which this effect depends.

The sparks produced by the collision of flint and steel, are probably owing to two causes. 1st, To the hardness of the stone, which enables it to penetrate the steel, and to detach from it very thin fragments in the form of ribands. 2ndly. To the combustibleness of steel. This metal being raised by the friction to a degree of temperature in which it is able to decompose the *oxygenous gas*, and offering by its great attenuation a considerable surface to the ambient atmos-

atmospherical air, combustion begins in some part of that surface, and once begun, the *freed caloric* is sufficient to continue it, till the metal becomes entirely *oxidated* or converted into a black ethiops.

This is therefore a chemical phenomenon, which does not take place in *vacuo*, because there is not any *oxygenous gas* there.

§ 162. It is perhaps allowable to <sup>Conjec-</sup> risk conjectures on a new subject, <sup>tures,</sup> when the truth is yet undiscovered.

May not the heat produced by a cause so similar to that which generates electricity, depend upon the electric action, which was, without doubt, excited by the gentle friction of the cotton against the surface of the glass bulb? Fire and the electric fluid

fluid develop themselves respectively in many other cases, and we have seen an example in the preceding experiments, which may assist us in conceiving the possibility of a reciprocal action of the two fluids in this last.

If electricity should have no share in these effects, may not a vibratory movement, excited by friction in the matter of fire contained in the pores of the substances, develop the action of this element so eminently elastic ?

Thus, to give an example of an analogous effect, the wet finger moved lightly on the edge of a glass produces a stronger and more complete sound, than a blow from a hard and solid body upon the same glass. In like manner the bow produces a stronger sound from the strings of a violin, by being drawn

drawn across them, than can be obtained by striking them violently.

A particular fact observed by Mr. Benjamin Thompson \* tends to confirm this conjecture. He remarked, in a course of experiments on some pieces of Ordnance, that the cannon was much more heated when fired with powder only, than when a ball was added; and he shews very clearly, that the heat which the piece possesses after the discharge, is, in general, very little owing to the heat produced by the inflammation of the powder. That able philosopher endeavours to explain this fact by the hypothesis which I have just mentioned. He presumes that the sudden commotion which takes place at the instant of

\* Philos. Transact. 1781, 2nd part.

the explosion of the powder without ball, puts the fire much more effectually into vibration, than when the shock is deadened by the presence of the ball.

But what is the quality of bodies most likely to produce this vibration in the element of fire? Is it their own elasticity? The observations I have made seem to prove the contrary. Do not the specific heat of various substances and their different permeability to the matter of fire operate also as coefficients in the effects of friction? and if so, experimental inquiries become complicated and very difficult.



